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THE EXTRAORDINARY SUNSETS.

BY A. COWPER RANYARD.

THE information which has come to hand in the course of the last three months shows that the extraordinary sunset and sunrise phenomena, which first attracted attention in England at the beginning of November, have been equally attracting attention in Australia, Japan, South Africa, India, and North and South America; and that the area within which blue and green suns and extraordinary sunsets were first observed, had some relation to Krakatoa as a centre. But there is still a great deal which requires explanation, especially as to the way in which the Krakatoa dust-cloud spread, apparently against prevailing winds.

There can be little doubt that the blue and green appearance of the sun observed in tropical countries for a few weeks after the great eruption was caused by dust floating in the atmosphere. Similar appearances have frequently been observed during dust-storms. Professor Piazzi Smyth, in Vol. XIV. of the "Edinburgh Observations," gives a coloured picture, showing what he describes as a blue sun. The sketch was made on entering Palermo Bay on the evening of March 10, 1872, when the sirocco wind, laden with fine dust from the African desert, was blowing.

Mr. Edward Whymper, while ascending Chimborazo on July 3rd, 1880, saw an eruption of Cotopaxi, 65 miles distant. A column of smoke of inky-blackness, as seen against the sky, rose into the air with prodigious velocity. He estimates that it rose 20,000 feet above the rim of the crater in less than a minute; it was then borne away parallel to the horizon, and ultimately passed over Chimborazo between the observers and the sun. Mr. Whymper, in describing the appearance in *Nature*, Vol. XXIX., p. 199, says, "Several hours passed before the ash commenced to intervene between the sun and ourselves, and when it did so we witnessed effects which simply amazed us. We saw a green sun, and such a green as we have never either before or since seen in the heavens. We saw patches or smears of something like verdigris-green in the sky, and they changed to equally extreme blood-red or to coarse brickdust-red, and they in an instant passed to the colour of tarnished-copper or shining brass."

Mr. G. F. Chambers, at the January meeting of the Astronomical Society, stated that the engineer of some works at Eastbourne, at which large quantities of seabeach are crushed by steam machinery, had informed him that he had frequently seen the sun appear blue and green through the fine dust which rises into the air when the operations are in progress. Dr. Budde, of Constantinople, writing to *Nature* of Dec. 20, states that when in Southern Algeria, he heard a Frenchman exclaim, "C'est la première fois que j'ai vu le soleil bleu." Upon asking others present, "he was assured by the whole company that the sun, seen through the fine dust of a Sahara wind, had a decidedly blue colour." I will give one more quotation in proof of the connection between dust floating in the atmosphere and the blue appearance of the sun. Mr. O'Reilly, writing to *Nature* of Jan. 17, quotes Richtofen's work on China, Vol. I., p. 97. After describing the dust-laden atmosphere of the Loes district, he states that for many days the air often appears yellow and opaque, "the view is completely hemmed in, and the sun appears as a dull bluish disc."

When the accounts of the blue and green appearance of the sun, observed in India, first arrived in England, it was suggested that the appearance was due to the absorption of the sun's rays by aqueous vapour which had been thrown into the upper air during the eruption of Krakatoa. Mr. Lockyer stated that he had seen "the sun a brilliant green through steam blowing off in great quantities from a little steamer on Windermere; and again, when it was seen through a thick mist on the summit of the Simplon Pass." I have several times looked at the sun through steam, but have not succeeded in seeing such an appearance. If aqueous vapour were present in the upper air in the condition of steam, the heavens would appear covered with cloud; but the blue appearance of the sun is not described as having been seen through cloud. Indeed, many of the observers are careful to state that there was no cloud. We have in England, in winter, plenty of opportunities of observing the sun through an atmosphere charged to various degrees of saturation with aqueous vapour, but the sun does not appear blue.

In his article in the *Times*, Mr. Lockyer assumes that the green colour was caused by "blue and red molecules" floating in the air. He states that the "red molecules are coarser than the blue ones, and would therefore be the first to fall from the upper air as dust." But it is not necessary to assume that matter of an exquisite blue colour was floating in the air. No such blue or red matter fell in sufficient quantities to be noticeable.

The blue colour of the sun, when seen through an atmosphere laden with fine dust, may be explained by simple physical considerations. A particle whose diameter is small compared with the wave-length of light disperses different proportions of red and blue light—the longer the wave-length the less is the intensity of the dispersed light. Lord Rayleigh has shown, in a paper "On the Light of the Sky," published in the February number of the *Phil. Mag.* for 1871, that when light is scattered by small particles the intensity of the scattered light varies inversely as the fourth power of the wave-length. We are all familiar with the blue colour of the heavens, and of water in which fine particles are suspended. Under ordinary circumstances, the light dispersed by the matter floating in the air is not sufficiently intense, sensibly to affect the colour of the direct light of the sun. But if the amount of fine dust floating in the atmosphere were greatly increased, the intensity of

the dispersed light would be increased, and the blue colour of the light dispersed from the part of the atmosphere between us and the sun would sensibly affect the colour of the sun.

When the dust particles are small compared with the wave-length, the colour of the dust does not sensibly affect the colour of the dispersed light. The phenomena seen by Mr. Whymper from Chimborazo was caused by comparatively large dust particles, in which the colours of the transmitted ray seems to have been red. The dust fell upon Mr. Whymper's party, and he brought specimens of it home for examination. He estimated that the finer particles weighed about the 1-25,000th part of a grain.*

There are two reasons why the sun does not appear blue every evening when as it sets the rays pass through a long distance of dust-laden atmosphere. In the first place, the particles floating in the atmosphere near to the surface of the earth are, many of them, not small compared to the wave length of light, so that the greater part of the light dispersed by particles in the lower air, is dispersed by particles which are large compared with the wave-lengths of the visible spectrum; and next, the aqueous vapour and other gaseous constituents of the atmosphere absorb more light at the blue end of the visible spectrum than at the red, so that in the long passage through the atmosphere the loss of light at the blue end of the spectrum more than counterbalances the bluish colouring caused by the light dispersed from such fine particles as float in the lower atmosphere.

In the tropics, shortly after the Krakatoa eruption, when the air was laden with fine dust, the blue sun became green as it neared the horizon, and ultimately it sank as a reddish ball, the absorption at the blue end at last entirely overpowering the admixture of blue colour.

There can be little doubt that the brilliant sunset colours observed in Europe and other places distant from Krakatoa are due to the ordinary atmospheric absorption, chiefly at the blue end of the spectrum, of the light dispersed by dust floating at a great height in the air in smaller quantities than in the tropical regions where the blue and green sun phenomena of the first few weeks after the eruption were seen.

Some experiments carefully made by Prof. Tyndall, render it probable that under ordinary circumstances terrestrial dust is not carried to great altitudes. In September, 1877, he opened on the summit of the Bel Alp, in Switzerland, 27 flasks containing infusions of beef, mutton, turnip, &c., which had been boiled for five minutes in London, and sealed during ebullition. A gentle breeze was blowing at the time from mountains which were partly covered with snow, towards the precipice where the experiment was made. Prof. Tyndall kept his body to the leeward of the flasks, and care was taken to cleanse the pliers with which the sealed ends were snipped off in the flame of a spirit-lamp.

Twenty-three similar flasks were taken immediately afterwards into a shed containing some fresh hay, and their sealed ends were then snipped off with the same pair of pliers. The two groups of flasks were then placed in a kitchen, where the temperature varied between 65° and 90° Fahrenheit. All the flasks opened on the edge of the precipice remained as clear as distilled water, while twenty-one of the twenty-three flasks opened in the hayloft became turbid and filled with organisms. This experiment confirms a similar experiment made by Pasteur on the Mer de Glace, and shows that terrestrial dust is not under ordinary circumstances carried by the winds to great altitudes, for the germs are so minute that they would certainly be carried upward by ascending currents if terrestrial dust were carried upward. It cannot be supposed that the cold experienced at such altitudes kills them, for laboratory experiments show that the germs survive intense cold, and that long exposure to the atmosphere and drying does not kill them.

But there is always dust floating in the upper air. This is proved by the radial polarisation of the light of the sky, as seen from high mountains. In 1878 I examined the polarisation of the sky as seen from Gray's Peak, in Colorado, 14,450 feet above the sea. The heavens appeared a deep, dark blue, but by no means black. There was sufficient light from the sky to render visible a small piece of white paper at the bottom of a brass tube, when the tube was held at right-angles to the sun's rays. The polarisation of the sky showed that the light from it was dispersed by very fine particles. The phenomena of twilight also show, that the sun's light is dispersed by particles floating at a great height in the atmosphere.

Where does this dust in the upper air come from? Till recently I thought that it could not come from below, and, therefore, argued that the fine dust of the upper air must be entirely due to the *débris* of the millions of small meteors encountered by the earth in the course of the year, and burnt in the upper air? The greater number of them are burnt out at a height of more than sixty miles above the sea level; and I thought, and still think, that their ashes may, owing to the fine powder to which they are reduced, take years in sinking to the earth. But the facts which have come to light with regard to the Krakatoa dust-cloud have shaken my faith in the conclusion which I had previously come to, and had begun to treat as an axiom, that the dust of the upper air must come entirely from outside space.

(*To be continued.*)

* Taking the scientific gravity of the matter ejected from the volcano as 2·5, a cubic inch would weigh 630 grains; and a little cube of such matter, weighing 1-25,000th of a grain, would measure along its edges 1-250th of an inch, a quantity which is very large compared with the wave-lengths of the visible spectrum.

THE CHEMISTRY OF COOKERY.

XXX.

By W. Mattieu Williams.

THE changes which occur when starch granules are subjected to the action of water, at a temperature of 140°, have been described. If the heat is raised to the boiling point, and the boiling continued, the gelatinous mass becomes thicker and thicker; and if there are more than fifty parts of water to one of starch a separation takes place, the starch settling down with its fifty parts of water, the excess of water standing above it. Carefully-dried starch may be heated to above 300° without becoming soluble, but at 400° a remarkable change commences. The same occurs to ordinary commercial starch at 320°, the difference evidently depending on the water retained by it. If the heat is continued a little beyond this it is converted into *dextrin*, otherwise named "British gum," "gommeline," "starch gum," and "Alsace gum," from its resemblance to gum-arabic, for which it is now very extensively substituted. Solutions of this in bottles are sold in the stationers' shops under various names for desk uses.

The remarkable feature of this conversion of starch into dextrin is that it is accompanied by no change of chemical composition. Starch is composed of six equivalents of

carbon, ten of hydrogen, and five of oxygen. $C_6H_{10}O_5$, *i.e.*, six of carbon and five of water or its elements. Dextrin has exactly the same composition; so also has gum-arabic when purified. But their properties differ considerably. Starch, as everybody knows, when dried, is white, and opaque and pulverent; dextrin, similarly dried, is transparent and brittle; gum-arabic the same. If a piece of starch, or a solution of starch, is touched by a solution of iodine, it becomes blue almost to blackness, if the solution is strong; no such change occurs when the iodine solution is added to dextrin or gum. A solution of dextrin when mixed with potash changes to a rich blue colour when a little sulphate of copper is added; no such effect is produced by gum-arabic, and thus we have an easy test for distinguishing between true and fictitious gum-arabic.

The technical name for describing this persistence of composition with changes of properties is *isomerism*, and bodies thus related are said to be *isomeric* with each other. Another distinguishing characteristic of dextrin is that it produces a right-handed rotation on a ray of polarised light, hence its name, from *dexter*, the right.

The conversion of starch into dextrin is a very important element of the subject of vegetable cooking, inasmuch as starch food cannot be assimilated until this conversion has taken place, either before or after we eat it. I will therefore describe other methods by which this change may be effected.

If starch be boiled in a dilute solution of almost any acid, it is converted into dextrin. A solution containing less than one per cent. of sulphuric or nitric acid is sufficiently strong for this purpose. One method of commercial manufacture (Payen's) is to moisten 10 parts of starch with 3 of water, containing $\frac{1}{150}$th of its weight of nitric acid, spreading the paste upon shelves, allowing it to dry in the air, and then heating it for an hour-and-a-half at about 240° F.

But the most remarkable and interesting agent in effecting this conversion is *diastase*. It is one of those mysterious compounds which have received the general name of "ferments." They are disturbers of chemical peace, molecular agitators that initiate chemical revolutions, which may be beneficent or very mischievous. The morbific matter of contagious diseases, the venom of snake-bite, and a multitude of other poisons, are ferments. Yeast is a familiar example of a ferment, and one that is the best understood. I must not be tempted into a dissertation on this subject, but may merely remark that modern research indicates that many of these ferments are microscopic creatures, linking the vegetable with the animal world; they may be described as living things, seeing that they grow from germs and generate other germs that produce their like. Where this is proven, we can understand how a minute germ may, by falling upon suitable nourishment, increase and multiply, and thus effect upon large quantities of matter the chemical revolution above named.

I have already described the action of rennet upon milk, and the very small quantity which produces coagulation. There appears to be no intercession of living microbia in this case, nor have any been yet demonstrated to constitute the ferment of diastase, though they may be suspected. Be this as it may, diastase is a most beneficent ferment. It communicates to the infant plant its first breath of active life, and operates in the very first stage of animal digestion.

In a grain of wheat, for example, the embryo is surrounded with its first food. While the seed remains dry above ground there is no assimilation of the insoluble starch or gluten, no growth, nor other sign of life. But when the seed is moistened and warmed, the starch is changed to dextrin by the action of diastase, and the dextrin is further converted into sugar. The food of the germ thus gradually rendered soluble penetrates its tissues; it is thereby fed and grows, unfolds its first leaf upwards, throws downward its first rootlet, still feeding on the converted starch until it has developed the organs by which it can feed on the carbonic acid of the air and the soluble minerals of the soil. But for the original insolubility of the starch it would be washed away into the soil, and wasted ere the germ could absorb it. The malster by artificial heat and moisture hastens this formation of dextrin and sugar; then by a roasting heat kills the baby plant just as it is breaking through the seed-sheath. Blue Ribbon orators miss a point in failing to notice this. It would be quite in their line to denounce with scathing eloquence such heartless infanticide.

Diastase may be obtained by simply grinding freshly germinated barley or malt, moistening it with half its weight of warm water, allowing it to stand, and then pressing out the liquid. One part of diastase is sufficient to convert 2,000 parts of starch into dextrin, and from dextrin to sugar, if the action is continued. The most favourable temperature for this is from 140° to 150° Fahr. The action ceases if the temperature be raised to the boiling point.

The starch which we take so abundantly as food appears to have no more food-value to us than to the vegetable germ until the conversion into dextrin or sugar is effected. From what I have already stated concerning the action of heat upon starch, it is evident that this conversion is more or less effected in some processes of cookery. In the baking of bread an incipient conversion probably occurs throughout the loaf, while in the crust it is carried so far as to completely change most of the starch into dextrin, and some into sugar. Those of us who can remember our bread-and-milk may not have forgotten the gummy character of the crust when soaked. This may be felt by simply moistening a piece of crust in hot water and rubbing it between the fingers. A certain degree of sweetness may also be detected, though disguised by the bitterness of the caramel, which is also there.

The final conversion of starch food into dextrin and sugar is effected in the course of digestion, especially, as already stated, in the first stage—that of insalivation. Saliva contains a kind of diastase, which has received the name of *salivary diastase* and *mucin*. It does not appear to be exactly the same substance as vegetable diastase, though its action is similar. It is most abundantly secreted by herbivorous animals, especially by ruminating animals. Its comparative deficiency in carnivorous animals is shown by the fact that if vegetable matter is mixed with their food, starch passes through them unaltered.

Some time is required for the conversion of the starch by this animal diastase, and in some animals there is a special laboratory or kitchen for effecting this preliminary cookery of vegetable food. Ruminating animals have a special stomach cavity for this purpose in which the food, after mastication, is held for some time and kept warm before passing into the cavity which secretes the gastric juice. The crop of grain-eating birds appears to perform a similar function. It is there mixed with a secretion corresponding to saliva, and is thus partially malted,—in this case *before* mastication in the gizzard.

At a later stage of digestion, the starch that has escaped conversion by the saliva is again subjected to the action of animal diastase contained in the pancreatic juice, which is very similar to saliva.

It is a fair inference from these facts that creatures like

ourselves, who are not provided with a crop or compound stomach, and manifestly secrete less saliva than horses or other grain-munching animals, require some preliminary assistance when we adopt graminivorous habits; and one part of the business of cookery is to supply such preliminary treatment to the oats, barley, wheat, maize, peas, beans, &c., which we cultivate and use for food.

HOW TO GET STRONG.

(*Continued from page* 136.)

MUSCLES OF THE WAIST.

IN dealing before with the muscles of the waist, I considered those in front chiefly, and the exercises then mentioned were intended to strengthen and tauten those important and usually much-neglected muscles. I have still to consider other waist muscles, and also to inquire what exercises are suitable for extending and rendering more elastic those same muscles for whose strengthening I have suggested appropriate measures.

First let me add to the exercises I before mentioned rowing, jumping, vaulting, and leaping. I am told that mowing acts very effectively to strengthen and harden the abdominal muscles, and I can well believe it; but as I have never mowed a square yard in my life, I cannot answer from experience. I should take some exception to mowing on the ground of its one-sidedness. For symmetrical development of the body, and for the increase of grace and lissomeness of movement, which I take to be much more important points than mere local development of strength, all exercises should admit of being interchanged from right to left. Thus the movements of fencing, though not conveniently altered in actual foil play (for every fencer will in actual contests put his best hand foremost), should be systematically repeated with the foil or a substitute for it in the left hand, and the right leg supporting the body's weight. So with sparring exercises, and others in which usually the sides of the body are differently employed. But in mowing one cannot well reverse the action, without having a scythe specially prepared for left-handed work. However, very few persons care to practise mowing, so this particular objection need not trouble us much.

There is one very trying exercise for the abdominal muscles which I would not recommend any except those already very strong-stomached to attempt. Sit on one of the parallel bars and carefully place both feet under the other; now steadily lower the body backwards, until you are lying horizontally athwart one of the bars and prevented only from going over by the hold of your feet under the other. Now steadily raise the body to the sitting position from which you started. This exercise may seem in effect much the same as one already given, in which sitting on a bed with legs horizontal and the feet placed under the lower bar of the foot-rail of the bed, you lower the body till it is horizontal and then raise it again—repeating as often as you may find convenient (not too often at first). But as a matter of fact you will find the parallel bar form of the exercise very much more trying than the other. Moreover on a bed if you feel unwilling to make a final rise from the horizontal position, all you have to do is to lie still and rest. But if on the parallel bars you lower yourself after you are nearly used up you cannot well avoid the effort necessary to come upright again. You certainly cannot rest comfortably across a horizontal bar, with so much of your weight on one side of it that only the catch of the feet under the other keeps you from falling over backwards. On the whole, I refrain from advising this particular exercise for hardening the abdominal muscles. When they are hard enough to stand it they are about as hard as you need them in ordinary life; and it should be remembered that exercise given unnecessarily to a set of muscles already well developed may be regarded in most cases as exercise unwisely withheld from muscles which need strengthening.

And now to consider those exercises which instead of giving the abdominal muscles energetic contractile work tend to stretch them and limber them up. These are especially desirable for all who lead a sedentary life; for the abdominal muscles get contracted and weakened in the sitting attitude, and especially in sitting over a desk. For this reason, and because there is no time so good for spinal exercises as the very time when undue rest resulting in the contraction of muscles has to be corrected, I give as many exercises as possible for stretching and limbering the waist muscles without apparatus or occasion to leave the room,—sitting-room, study, or even counting-house,—where the abdominal muscles are losing strength and activity.

It is a natural instinct after long sitting over a desk to extend the arms outwards upwards and backwards, throwing the shoulders back, and projecting the stomach a little forward. It is the movement seen in vigorous yawning; and many restrain it in themselves or check it in others as if it meant laziness. Probably if a merchant were to see half-a-dozen of his clerks simultaneously engaged in this natural and wholesome movement, he would be disposed to feel angry with them,—the action *is* so lazy looking. But if he knew what was good for them and therefore for him, so far from checking these movements, he would encourage his clerks to systematic stretching of this wholesome kind. To take off the lazy look he might invite them to get off their high stools, and standing well erect go through the backward stretching steadily and strongly several times in succession before resuming their sedentary work.

As a muscular exercise this stretching is excellent, and any one who has an artistic eye, and notes carefully from week to week the effect which it has on the figure of the chest, shoulders, and abdomen, will see that it is a beautifying exercise also.

Let us consider how the stretching, to be most effective, should be managed.

Stand well erect, with arms hanging straight down, and the feet somewhat apart to give a firmer stand. Draw a long breath, continuing the inspiration while the arms in the movement to be described are moving so as to expand and deepen the chest. Bring the arms from the sides steadily forwards, upwards, backwards, and then downwards, with the backs of the hands towards the ground. Breathe out now slowly, letting the arms sink still lower, but keeping them as close together as they can be brought in their backward position,—till they are hanging straight down as at first. Repeat the process four or five times, being careful to take as full a breath as possible in the first part of the stretching movement and to breathe out as fully as possible during the latter part.

Next slightly modify the exercise. Stand erect as before. Then, keeping the arms down incline the head and shoulders backwards as far as possible, arching the body forwards and hollowing the back, until the hands go as near as they can go to the heels. Recover slowly and steadily the erect position. Repeat this exercise half-a-dozen times,—or a dozen if you feel equal to it. If you give a little time and attention to this exercise every day you will not only find the abdominal muscles wonderfully improved, but that your lower limbs have felt the strain and benefited by the exercise.

On the stage you may see the extent to which, by such backward bending, the muscles of the abdomen may be stretched and made elastic. But it is not essential to your happiness, or the happiness of those around you, that you should be able to touch your heels with your head, or going a little farther that way, to bring your head forward between your feet and smile upon your friends from that comparatively low level. But though this, judged by Mr. Foster's rule, is not necessary or even desirable (for as Bishop Peter of Rum-ti-foo remarked, such contortions might pain your friends very much) yet the abdominal muscles cramped by much sitting over books and manuscripts, will be all the better for so much of this sort of exercise as may stretch and well loosen them.

There are many other exercises without apparatus or with only dumb-bells or clubs which have an excellent effect in the same direction.

In using clubs (Indian) for the backward swing, the ordinary way, and an excellent way for its particular end, is to keep the body rigidly upright. The work then tells chiefly on the chest, shoulders, and arms; in so far as it acts at all on the waist muscles it tends rather to harden and contract than to limber them. But now change the mode of swinging, letting the body yield as the clubs are swung forward overhead and backward,—so that as the clubs end their backward sweep the body is well arched forwards and the ends of the clubs almost strike the calves or even the heels. (But avoid actually striking, for while the blow will cause discomfort, it will not help the development of any particular set of muscles). Now after the full backward swing has thus been reached, swing the clubs well over the head to the front, making all the stretched muscles of the front of the waist take part in the beginning at least of the pull. Do not mind the shoulders and chest coming well forward as the clubs swing down past the feet and backwards. In fact throughout *this* club exercise give up the firm upright position which is essential in the ordinary form of the overhead swing. Make your body bend well forward as the clubs come down in front,—even so far if you like that the swing of the club backwards beyond the feet may be necessary to save you from toppling over forwards. Contrariwise let your abdomen come well forward as you send the clubs backwards over your head,—so far that there is an elastic backpull available from the abdominal muscles as you begin to bring the clubs up again.

This exercise has a splendid effect in correcting the defects of most of our English exercises. You will find the arms get less work than you might expect, though they are not idle. The shoulders are well worked; but the waist muscles, front and back, are those most benefited. (We will look after the side waist-muscles later on.) The hardest work of all, however, is generally done below the knee in this fine exercise.

The dumb-bells can be similarly used with a swaying body, bending forwards and arching backwards alternately. But they do not give the same pleasant sensation as well chosen Indian clubs (not too long). Growing lads in particular get much more benefit from the swaying exercise with the clubs than with the dumb-bells. Indeed, heavy dumb-bells are apt to check growth.

Blackie mentions rightly as good for the extension and limbering of the abdominal muscles,—"All work above the head, such as swinging an axe or sledge; putting up dumb-bells, especially when both hands go up together; swinging by the hand from a rope or a bar or pulling the body up till the chin touches the hands; standing with back to the pulley weights" (described in former papers), "and taking the handles in the hands and starting with them high over the head, then pushing the hands far out forward; standing two or more feet from the wall and placing the hands side by side against it almost as high up as your shoulders, then throwing the chest as far forward as possible," besides the work of special trades, such as ceiling work by the plasterer and painter, hauling down ropes by sailors, and so forth.

Sparring and fencing are both excellent exercises for the front abdominal muscles. You get the best exercise in this way, alone; because you can work systematically using right and left sides equally. The part of the work which tells most in the way we are considering is not the lunge in fencing or the delivery of the blow in sparring, but the recovery in the former and the drawing backward of head and shoulders in the latter. Quickness in all such movements is well worth acquiring, apart from mere muscular development.

A capital plan for sparring is to face a mirror too far from you to be struck, and to deliver your blow sharply in such a way that your fist hides whatever part of your image in the glass you may wish (as forehead, eye, nose, mouth, neck or chin); then recover sharply, bring the striking arm (elbow bent) down to your side, and throwing up the other sharply as if to ward a blow aimed at your face.

(To be continued.)

A YEAR'S WEATHER FORECASTS.

By JOHN W. STANIFORTH.

PART II.—THE WEATHER.

THE forecasts of the general state of the weather may be considered under the heads of rainfall, temperature, and humidity.

(*a*) *Rainfall.*—Occasionally rain is predicted in such definite terms as "rain at times," "some rain," "showery," but often it is only vaguely indicated by such nondescript terms as "changeable," or "unsettled." In analysing the forecasts for rain, I have only taken account of the former class, and I find that on 167 days rain or snow was confidently predicted. The rain or snow fell on 120 of these days, but failed to put in an appearance on the remaining 47. In addition, rain or snow fell on sixty days when the prophecy led us to expect neither.

(*b*) *Temperature.*—Changes in temperature are not often predicted, the office contenting itself with such terms as "cool" or "warm." Occasionally, however, a sudden change is accurately foretold, as on April 6 and 7, when the forecast for the 6th said "colder" and that for the 7th "warmer," which was exactly what happened, the thermometer falling as low as 30° F. on the 6th, but rapidly rising again on the 7th. Altogether 69 forecasts were made with regard to temperature, of which number 58 proved correct.

(*c*) *Humidity.*—Only twice (April 23 and May 16) did the office venture to predict a *dry* state of the atmosphere, and on the first occasion the relative humidity, as indicated by the wet and dry bulb thermometers, was 91 at noon and 100 at 6 p.m. On the second occasion, however, there *was* a minimum quantity of watery vapour in the air, the relative humidity being 57 at noon and 73 at 6 p.m. With the opposite condition the office was a little more fortunate, predicting fog, mist, or haze on 42 occasions and being correct on 24.

Taking these factors into consideration, together with such conditions as "dull," "fair on the whole," "cloudy," "thunder," &c., I valued the general weather forecasts (if I

may so call them) under the heads of correct (C^3), moderately correct (C^2), and incorrect (C^1). These letters were entered after the A's and B's, so that each day the full value of the forecast was ascertained and indicated by such formulæ as $A^4\ B^4\ C^3$ (maximum of correctness) $A^4\ B^3\ C^1$, &c. I may add that in no case was the forecast ever consulted before writing out the report—a plan which is, in my opinion, very necessary if an impartial test is to be made. In the following table is given the success achieved by these forecasts for each month of 1883.

	C^3	C^2	C^1
Jan.	15	7	5
Feb.	13	8	3
March	14	6	7
April	10	8	7
May	14	7	6
June	15	9	2
July	13	8	5
Aug.	12	7	8
Sept.	16	6	3
Oct.	16	8	3
Nov.	16	3	7
Dec.	15	7	3
Totals	169	84	59

Summarising these results, we find the office has been successful in nearly 70 per cent. of its forecasts of the wind's direction, in 60 per cent. of those relating to the wind's force, and in slightly less than 55 per cent. of prophecies of the general weather.

What is the conclusion to be drawn from these figures? It is, I think, that the office may fairly claim our confidence in its wind predictions, but cannot justly claim it for forecasts which are right in 55 per cent., doubtful in 25, and incorrect in 20.

WEEVILS.

By E. A. Butler, B.A., B.Sc.

AMONGST the great order Coleoptera, or beetles, there is no more destructive group than the weevils. They are vegetable-feeders, and some of them severely attack many of the objects of cultivation in this country, doing a considerable amount of damage, especially during their larval existence. Nearly 500 species have been recorded as belonging to the British Fauna, but these do not attain any great size; only one or two are over half an inch long, and the majority range from quarter-inch downwards, the smallest being only one-twenty-fourth-inch in length. Their shape is remarkable and characteristic. The head is produced into a snout or beak—technically, the *rostrum*—which is sometimes short and thick, but frequently long, slender, and arched, and in a few instances even as long as all the rest of the body together (Fig. 1). This apparatus which, because of its proceeding from between the eyes, looks like a nose, though it certainly does not perform the functions of such an organ, being used neither for breathing nor smelling, carries at its apex the organs of the mouth, which, throughout this group, are reduced to small dimensions, though similar in general plan to those of ladybirds, described in a former paper. It is obvious, therefore, that every particle of food taken by the insect has to traverse the whole length of the rostrum before it gets anywhere near that part of the alimentary canal where the stomach is situated. To the sides of the rostrum are attached the antennæ, composed of from nine to twelve joints, the basal one of which is generally, though not always, so much larger than the others as to form nearly half the length of the entire organ; the terminal part, consisting of some slender joints followed by a club or knob, can be bent at an angle to the basal joint, thus making what is called an elbowed antenna. The whole antenna can be folded up after the manner of a pocket-map, the terminal part being bent back upon the basal joint, and this again upon the rostrum, on each side of which a groove is excavated for its reception. The thorax is either like a truncated cone, or nearly globular, and has no sharp edge at the sides, as most beetles have. The elytra cover the greater part or the whole of the abdomen, and are always present, though in many cases there are no wings for them to protect. The whole body is often covered more or less closely with tiny scales, which, being variously coloured, white, yellow, red, black, green, and so on, add greatly to the beauty of the insect. Some few are clothed with frosted green scales, which glow and sparkle in the sunshine, and render the creatures dazzlingly beautiful. This beauty, when viewed by the aid of reflected light under the microscope, becomes a perfect blaze of splendour, which reaches its climax in the well-known exotic Diamond Beetle, a member of this section. The scales sometimes form a pattern on the elytra; they are much too small to be individually distinguished by the naked eye; but put the insect under the microscope, and that which before appeared simply a variegated design, is seen to be a lovely mosaic composed of an immense

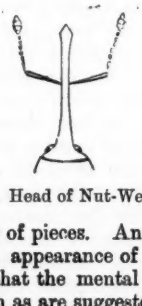


Fig. 1. Head of Nut-Weevil.

Fig. 2. Sitones lineatus.

number of pieces. And here, let me say, that the microscopical appearance of insects is so familiar to an entomologist, that the mental images he habitually forms of them are such as are suggested by this, and not by their ordinary appearance, and his descriptions of beauties of form and colour are apt to appear somewhat overdrawn to one who sees the things only in the insignificance in which they appear to the eye uninstructed by the revelations of the microscope. The scales are not very strongly attached, and the beetles, after having done a little knocking about the world, look decidedly the worse for wear; the dull-black or dark-brown of their bodies showing here and there, where the scales have been worn off, gives them a very "seedy" appearance, when compared with the dandified look of the freshly disclosed insect. The legs are stout and the feet broad, an arrangement which enables the creatures to take a firm foothold upon the vegetation on which they live.

A good notion of a weevil larva may be obtained from the fat, whitish, footless, wriggling maggots which often, unpleasantly enough, take the place of the kernel in nuts and filberts: these are the larvæ of the nut-weevil, concerning which more anon. The pupæ, as with beetles generally, show distinctly all the parts of the future insect, the beak being bent under and folded lengthwise along the breast.

Peas, beans, and other leguminous crops, such as clovers and trefoils, are very subject to the attacks of weevils. There is a genus called *Apion*, containing nearly eighty British species of tiny beetles, the largest only about $\frac{1}{8}$ in.

in length, which are something like animated notes of exclamation (!) minus the dot, and plus six straggling legs and a pair of antennæ which are not elbowed. They have also been not inaptly compared to pears and to pegtops, the "peg" being the rostrum. They are mostly black or dark blue, and several are destructive to leguminous plants, in the seeds of which sometimes the larvæ live. They are gregarious, and many occur in fields and amongst rank vegetation in immense numbers, though they are far too small to be noticed without being specially looked for.

Some species of the genus *Sitones* (Fig. 2), a much larger set of insects, attaining the length of nearly ¼ in., are still more hostile to the leguminous crops. Nineteen species of this genus have been detected in this country, but most of them are so much alike that none but an experienced entomologist would be able at all easily to separate them. They are of an ochreous or clay colour, and yet this is not so; they are really black, but the whole body is so closely covered with ochreous scales that no trace of the ground-colour can be seen, except after the insect has suffered somewhat severely from the vicissitudes of fortune, and has lost its ornaments. They are a great contrast to the slender-snouted Apions, for they are cylindrical in shape, and the rostrum is extremely short and almost as broad as the head. Contrary to what is usually the case, they seem to be much more harmful in the perfect than in the larval state. In a field of peas, the leaves of the plants are often seen jagged or scalloped at the edges; this is the work of these weevils, and sometimes they carry their depredations so far as to leave nothing but the mid-rib and its branches to testify to the former presence of a leaf. They treat many other plants in the same way, and when not engaged in the business of their life, they may be found at the roots of plants, under stones, clods of earth, &c. It is not easy to find them actually feeding, as they are wary insects, and, on the approach of an observer, will suddenly fall to the ground as if dead, where they easily escape detection. In spring, large numbers may frequently be seen in towns, crawling about our walls, especially where gardens are attached to the houses.

(*To be continued*).

ELECTRO-PLATING.

I.

By W. Slingo.

THERE is, perhaps, in the whole range of electrical science no branch that is capable of offering greater or more varied attractions to the amateur or the student than that known as electro-chemistry, although, of course, in its deeper and more intricate paths there are theories and principles which demand much closer application than the amateur is, as a rule, able to give. Variety is itself a source of pleasure to the human mind, and renders gratifying that which would otherwise be looked upon as tedious and irksome, whilst there is also to be mentioned as an element of attraction the small amount of technical or purely scientific knowledge requisite to enable one to pursue the subject in its smoother paths. Nor, again, is the apparatus (without which the study of any branch of physical science must be extrememly monotonous) of an elaborate or expensive character. It is, however, essential, if good results are required, that the experimentalist's powers of observation should be called into play. Upon this a great deal depends. Especially on such points as the efficiency of any particular battery in use, the best relative constitution of the solutions, and the time taken to produce any required effect. It is no wonder that, with its many attractions, the first announcement of the effect of a current upon a sulphate of copper solution should have set the world a-going, repeating the announced experiments in a multitude of ways, which rapidly led up to most extensive business operations in the nobler metals as well as in copper. The almost universal practice of using plated instead of solid silver or gold articles is one which promises to extend very considerably, and that for many reasons; but perhaps the chiefest is one upon which those gentlemen who delight to go "a-burgling" can say most, as they have lost a very profitable source of income.

The subject of electro-chemistry is not by any means a new one, nor is that particular branch of it known under the general heading of "Electro-plating," which, for my purposes here, I shall use to embrace all those various applications of electricity in which a metal is deposited upon an "electrode" (a term used to indicate that portion of the solid conductor which is immersed in a liquid), whether that electrode be metal or otherwise. It is one of those subjects which seem to have been almost lost sight of in the general excitement produced by the re-introduction of the electric light, but it is one which, nevertheless, may be made to yield many a pleasant evening's amusement during the winter months, and, for that matter, during the summer months also.

The science of electro-chemistry might almost be styled the science of liquid electrical conductors, because, in the first place, the liquid must be a fair conductor, or the current will not traverse it, and therefore the liquid will not be affected; and in the second place, because there are very few liquids which do not suffer a chemical change on the passage of an electric current. The simplest form of change so produced is that resulting from the action of a current of electricity upon water, which is divided into its two constituent gases, oxygen and hydrogen. Although this is a commonly-known experiment, its lessons are but little known. It is a remarkable fact that electricity is the only form of force by which we can readily decompose water without giving the produced gases something else with which combination may take place, as happens when a piece of potassium or sodium is thrown on to water. In this case the water is decomposed by the metal, which combines with the oxygen and half the hydrogen, and releases the other half of hydrogen. An electric current effectually separates the two gases, and they may easily be collected if suitable apparatus is employed. While the recent extensive fire which destroyed several timber-stacks was raging a theory was started that the heat of the fire decomposed the water, and that the escaping inflammable hydrogen caught fire, and so "added fuel to the flames." I am not aware, however, that the theory received any measure of support. When an electric current decomposes a substance, the products are not necessarily equal in volume or in weight. Thus water, when decomposed between platinum electrodes, yields one volume of oxygen to two similar volumes of hydrogen, while the relative weights are as eight of the former to one of the latter. These results are governed by the proportions of the several elements which enter into the various combinations, and by the relative weights of the ultimate atoms of each element.

The decomposition of a liquid by the passage of an electric current is termed electrolysis, and is produced by dipping the wires or other conductors in connection with the battery into the liquid. It may happen that one of the wires or electrodes is a metal assailable by one of the products. Thus, if copper electrodes are used for the decomposition of water, the hydrogen may be col-

lected; but the oxygen combines with the copper, and forms an oxide of that metal. This fact is well worth remembering, because by its aid we may readily ascertain the previously unknown direction of a current flowing through a circuit. To do this, all that is necessary is to moisten the finger with saliva or any saline solution, and then place upon it the two copper wires of the broken circuit. Bubbles of gas will be seen to accumulate on one wire, which the other turns black, and it may be noticed that however many times we repeat the experiment the bubbles always appear on the wire in connection with the zinc pole of the battery, and these bubbles are of hydrogen gas. Consequently we may always rest assured that the positive pole is that one which oxidizes, and the current travels from it through the solution to the negative electrode. Where it is desired to collect oxygen, copper manifestly must not be used, but small pieces of platinum foil or wire instead; and similarly where chlorine is one of the products, platinum must be avoided, otherwise the platinum will combine with the chlorine, and platinum is 35s. per ounce. A very simple and instructive experiment may be made by passing a current through a solution of common salt (known chemically as sodic chloride, Na Cl) slightly coloured by litmus or indigo. The salt is separated into sodium and chlorine, the former forming with water caustic soda, and remaining in solution; the latter exercising the property, peculiar to chlorine and a few other substances, of bleaching the solution. The bleaching occurs in that portion of the solution surrounding the electrode in connecting with the positive pole of the battery, while the sodium appears at the electrode in connection with the negative or zinc pole of the battery. In this experiment the electrodes must be of carbon, because of the absence of chemical affinity between it and chlorine. If, again, a solution of potassic iodide (KI) be subjected to the passage of a current through it, it is decomposed, and the previously colourless solution is rendered brown in the vicinity of the positive electrode, due to the freed iodine; while the other constituent, potassium, is separated at the negative electrode, and, combining with the water, forms caustic potash, hydrogen being set free.

A somewhat similar, but perhaps more instructive experiment is to divide the electrolytic cell into two divisions by means of a piece of porous earthenware, or what will answer the purpose almost equally well, a few sheets of blotting-paper stitched together. Fill both with sodic sulphate solution, adding a few drops of litmus solution. To one division, that intended to contain the negative electrode, add two or three drops of sulphuric or hydrochloric acid, the effect of which is to redden the blue litmus solution. On the passage of the current the liquid surrounding the positive electrode becomes red, while that surrounding the negative electrode becomes blue. The reason of this is that the current decomposes the sulphate of sodium, the acid portion appearing at the positive electrode and the metal at the negative electrode. The acid portion (SO_4) combines with the hydrogen of the water, and so produces sulphuric acid ($SO_4 H_2$) while the other constituent of the water, oxygen, escapes (the electrodes being of platinum). The sodium acting upon the water forms caustic soda (NaHO) which is a substance known as an alkali, and has the property of rendering red vegetable solutions blue, being the opposite of acids which redden blue solutions. By subsequently reversing the electrodes the colours may again be made to change places. One important point to be gathered from this article is that in all cases the metal travels with the current, that is to say, it is liberated at the negative electrode.

The experiments above enumerated are all of a simple nature, and may be performed with the aid of a current produced from two good cells, Bunsen, Grove, Bichromate, &c., of moderate size—say one quart capacity.

THE UNITY OF NATURE.*

THIS is a book concerning which the time-honoured dictum may be emphatically repeated: that whatever in it is new is not true; and whatever is true is not new. Certainly it would have rejoiced the heart of the old lady who was so lost in admiration of that Providence which always caused large rivers to run by cities—if she could only have kept awake over it. Doubtless, too (similar conditions being pre-supposed), it will be regarded as more or less convincing by every one who is previously in agreement with its noble author. Whether, though, it will carry conviction to the minds, or in the slightest degree alter the views, of that vastly preponderating number of scientific men who differ from him, we venture gravely to doubt. Professedly theological in its end and aim, its avowed purpose would exclude it from notice in these columns; but for the fact that its illustrations are, or profess to be, drawn from the most recent results of scientific research. It is, then, to the Duke's treatment of these that we must perforce confine our criticism. The work before us contains evidence of a good deal of desultory reading; and of the amassing of a large number of imperfectly appreciated facts, mainly in Natural, as contradistinguished from Physical, science. Where any of these seemed to lend countenance to the thesis of the author, they have been quoted. In the cases in which they obviously militate against the conclusions he wishes to establish, they are either calmly ignored or (what is more annoying to the reader who demands argument and not assertion) pooh-pooed. His very eclectic quotations from Max Müller will serve as well as anything to illustrate what we mean by this. The House of Lords were told, on a memorable occasion, that they were "up in a balloon." Whatever may have happened to his Peers, it is tolerably evident that the Duke of Argyll is still so soaring, and that from this supreme elevation he looks down with a kind of wondering pity on men of such inferior intellectual and ratiocinative powers as Lyell, Huxley, Tyndall, Herbert Spencer, G. H. Lewes, Lubbock, Christy, Lartet, Pouchet, Wallace, Pengelly, and, the greatest philosophical naturalist the world has yet seen, Darwin—the results of whose imperishable labours are, it seems, to be overborne and thrust aside in deference to the opinions of a Mr. Dawson, who is apparently the head of some college in Montreal. What boots it that there is a practical consensus of opinion among our chief living embryologists as to the significance of rudimentary organs, and of the sequence in the development of the fœtus? The Duke (pp. 37 *et seq.*) knows better. To admit that Reason exists in the lower animals would seriously militate against the views advanced; as would assuredly the confession that they possess anything equivalent to language: so both are, not disproved, but denied (pp. 92 and 108). That "Conscience" may have had its origin—to put the matter in its curtest form—in our surroundings, would be a conception which would utterly fail to fit in with the author's argument. So this idea is also contumeliously dismissed. It would be a curious subject for inquiry, how, on the principles developed on pp. 322, 323, &c., of the work before us, the fact is explicable that thousands of women who would almost die

*"The Unity of Nature." By the Duke of Argyll. (London: Alex. Strahan.) 1884.

with shame at the idea of raising their dresses four inches above their ankles in the presence of a strange man, will yet lie, in the most strenuous manner, with unimpaired cheerfulness, when any end is to be gained by it. Among other assertions (for nothing worthy of the name of argument is vouchsafed) we find the predication of the origin of the entire human race from a single pair (pp. 386 and 398), as also that of the practically unchanged physical geography of the earth since man's first appearance upon it. It happens curiously that, as we write, the news comes of the arrival of Professor Hull, the Irish Director of the Geological Survey, from Palestine with his party, who have been engaged in making a geological survey and map of the Holy Land. We refer to this here, because Professor Hull gives it as his deliberate opinion that, at the date of the traditional Hebrew exodus from Egypt, the Mediterranean and the Red Sea were in continuous connection! So much for the historical immutability of the present distribution of land and water, insisted on on p. 403. Bare mention will suffice of the very far-fetched explanation of animal worship given on pp. 493 and 494; and of the employment of that stalest of all polemical devices, the imputation of moral obliquity on p. 528 to all who differ from the opinions of the Duke. "I wish," said Sydney Smith, apropos of Lord Macaulay; "I wish that I were as cock-sure of anything as Tom Macaulay is of everything." In this respect, though probably in this alone, the mantle of the great Whig historian certainly seems to have fallen on the Duke's shoulders. Amid this "intolerable quantity of sack," it would be strange indeed if the "ha'porth of bread" were not somewhere to be found; and there is one portion of "The Unity of Nature" which presents a certain amount of freshness, and something approaching to logical treatment. We refer to the section in which the question of the savagery or civilisation of primæval man is discussed. Dealing with the subject of the existence of such races, our author adduces the present condition and geographical location of the Esquimaux, the Fuegians, the Bosjesmans, &c., as affording cogent proof that they are merely the degenerate descendants of higher and more civilised nations, from which their progenitors have in times past been driven by war or cognate causes; and that hence, so far from the earliest human inhabitants of the earth having been savages, they were probably in reality vastly higher in the social and intellectual scale than many of their descendants. It is this portion of the book which relieves it from the level of dulness that rests like a pall over the other parts of it. The argument, even if it can scarcely be pronounced convincing, exhibits a certain amount of ingenuity; and will probably be read with more or less pleasure and interest by many for whom other parts of the work will possess but scant attraction. That, however, as we began by saying, any one will be convinced by this latest essay of the Duke of Argyll's, who did not agree with him previously, we regard as a matter of the gravest doubt. With perverted ingenuity he has treated a subject of the highest and most enduring interest in a fashion so dull and dreary as to be almost inconceivable by any one who will not take the pains—which we have done—to wade sedulously and conscientiously through its 552 pages. That, though, we may give it all the praise to which it is honestly entitled, we may add, in conclusion, that it is followed by a really good index.

The British Museum.—The Government have declined for the present to provide funds for lighting the British Museum by electric lamps, so that the keeping open of the building after dark has been deferred.

THE MORALITY OF HAPPINESS.

By Thomas Foster.

CARE FOR SELF AS A DUTY.

(*Continued from page* 123.)

It will perhaps be sufficient in response to numerous inquiries addressed to me respecting the supposed religious bearing of these papers to remark that they are not intended to have any religious bearing whatsoever. I am simply inquiring what are the rules of conduct suggested when each person takes as his guiding principle the increase of the happiness of those around, an expression which must be taken as including himself in the same somewhat Hibernian sense in which Milton included Adam among "those since born, his sons." I may add that nearly all the letters addressed to me have been interesting, and some have been singularly well-reasoned,—all utterly unlike the rather spiteful and very silly letters I referred to in a footnote to my last paper. Yet I cannot suffer the religious element to be imported into the subject,—no matter how courteously or kindly the thing may be done. I have just the same objection to see the question of the evolution of conduct considered from that side, which the student of astronomy or geology has against dealing with the objections and difficulties raised by those who seem always to suspect that under the teachings of God's work, the universe, there may lie some grievous deceptions if not some monstrous falsehoods. If my reasoning is bad, it can be met and overcome on its own ground.

I may, however, make this general remark with regard to all systems of morality whatsoever, including those which have come before men in company with religious teachings. Without a single exception every one of these systems includes—and professes to include—features suitable to the special time and the special place when and where it was propounded. How much of any system may thus be regarded as local or temporary or both may be a moot point; but that some of each system is of that sort is absolutely certain. "Because of the hardness" of men's hearts the Mosaic system for instance had certain rules; and because of the weakness of their hearts (who can doubt it?) the system which replaced that of Moses had certain other rules. The same is true of every system of conduct ever propounded. We may believe the rule sound and good in its own time and place, "Whosoever shall smite you on the right cheek turn to him the other also," and "If any man will sue thee at the law and take away thy coat, let him have thy cloak also." A man may believe these rules to be more than sound and good, to be of divine origin,—yet recognise that in our own time, and here, in Europe or America, the rules would work ill. He who so taught recognised in the same way that other rules which had been good in their time had lost their virtue with changing manners. He knew *where* it is written, "Thou shalt give life for life, eye for eye, tooth for tooth," and so on; yet he only quoted these Scripture teachings to correct them,—"But *I* say unto you, that ye resist not evil, but whosoever," &c. When he thus corrected what was "said by them of old time," he did not show disrespect—whatever the Scribes and Pharisees tried to make out—for the teachers of old time, whose words he read and expounded. He knew that "old times were changed," and therefore old manners and morals gone. He said "Suffer little children to come unto me," and loved them, *not* teaching—as had seemed more convenient and was (let us believe) better, in earlier days—that the child would be spoiled unless diligently belaboured with the rod.

These times and the races and nations now most prominent on the earth are even more unlike the community in Palestine nineteen centuries ago, than that community was unlike the Jewish people in the days of the more ancient lawgiver. The opponents of evolution may prefer to believe that the human race has been stereotyped; but facts are a little against them. And even if we admitted the imagined fixedness of the human race for nineteen centuries, they would still have to explain the contradiction between two systems for both of which they find the same authority. Of course there is no real or at least no necessary contradiction. Grant the human race to be what we know it to be, a constantly developing family, and the contradiction vanishes,—we simply learn that what is best for one time is not best for another, even among one and the same people; how much more then must the best rules of conduct vary when different peoples as well as different times are considered.

All this however is a digression, which should have been unnecessary but has in a sense been forced on me by the misapprehensions of many well-meaning critics, [and a few who are not well-meaning at all, but of the Honey-thunder order, teaching the law of love by reviling and worse].

The duty which each man owes to himself in regard to the maintenance of his health, the development of his powers and so forth, which becomes a duty to others when regarded with reference to those more immediately around him or dependent upon him, and is still manifestly a duty in relation to others where the advancement of the general well-being so far as he can influence it, is considered, has another aspect when considered in reference to those classes (D and E of our list, p. 122) whose encouragement or increase would be injurious to the body social. It is not only essential to the evolution of conduct in the right direction that those who may be classed as "men of good-will"* should increase relatively in number and influence, but also that those who are either absolutely men of ill-will, or are so far not of good-will that they disregard the well-being of others, should be checked and discouraged.

This requirement for the evolution of the more altruistic kind of conduct involves in many cases—as a duty—conduct of a kind which the few real members of class A and the many members of class C who speak of themselves as belonging to class A—regard as self-assertive. It becomes a duty, when the matter is viewed in this light, to assert just rights and resist wrongful claims. For, every act of carelessness or self-neglect in such matters tends to the encouragement of the less valuable or noxious classes which profit by it. It may be that to uphold just claims or resist wrongdoing may be less comfortable than to give way. In such a case the duty becomes an altruistic one, however egoistic the action based on the consideration of such duty may appear. But in a number of cases the claim upheld may be well worth upholding in itself, the wrong resisted may involve gross injury. In such a case the care of a personal right or the resistance of a wrong is, in itself, egoistic. Yet may it well be that the person concerned may esteem it better to give up the claim or to yield to the wrong, until he recognises that the idea of self-sacrifice, however beautiful in itself, may involve a far-reaching wrong to the better members of the body social.

We touch here on considerations which are in question every day, almost every hour, of our lives.

Consider home-life for example. In nearly every home there are those who are disposed to take unfair advantage of the rest; and they are far better restrained by the quiet resistance of their attempts than in any other way—certainly far better than by yielding, continued till nothing but the anger roused by some attempt, more barefaced than the rest, moves to resistance. We see this especially exemplified in the families of careless parents—unselfish perhaps in a sense, but really negligent of their duties. It has been said for this reason that unselfish parents have commonly selfish children, which seems contrary to the law of heredity, but illustrates rather the natural influence of defective training. The fact really is that the children of selfish parents are as a rule more selfish in character than those of the unselfish; they grow up to be as unpleasant in their ways as the children of careless, unwatchful parents; and their unpleasantness is more apt to be permanent. Yet the unchecked ways of children whose parents yield unwisely to them, illustrate well on a small scale (even though happily the mischief is often transient)—how the assertion of just claims, and the restraint of wrong-doing, involve a form of egoism which must be regarded as a duty.

In life outside the family, we constantly find the duty of resisting evil presenting itself in apparently egoistic aspect. In hundreds of ways the members of class C show their readiness to become members of class D and members of class D to develop their unpleasant ways. The adoption of considerate habits and care for the just claims of others in all the multitudinous details of our daily life, constantly lead to attempts by the selfish and obnoxious to take advantage of what they regard as mere weakness of disposition. In such cases while it is by no means desirable to give up ways which are in themselves essential to the well-being of the society of which we form part, we must—as a duty—resist the encroachments of objectionable persons,—not the less that the matter insisted upon is one to which we attach importance, so that our firmness has its egoistic aspect. Men are but children of a larger growth, and there is no surer or better way of eliminating at least the grosser forms of selfishness than by so resisting unjust claims that they—simply fail. This is the appropriate punishment,—akin to that which Mr. Spencer regards (most justly in my opinion) as the only proper form of punishment for children, viz., punishment which is the direct consequence of ill conduct. Of course, it will happen that mere resistance of a wrong may bring definite punishment—directly or indirectly—to the wrong-doer: but (apart from such cases, in which we have to ask whether justice may not need to be tempered with mercy) all I would insist on is that the selfish, grasping, oppressive members of the body social, should be so resisted that, whenever it is possible, they fail of their unfair purpose.

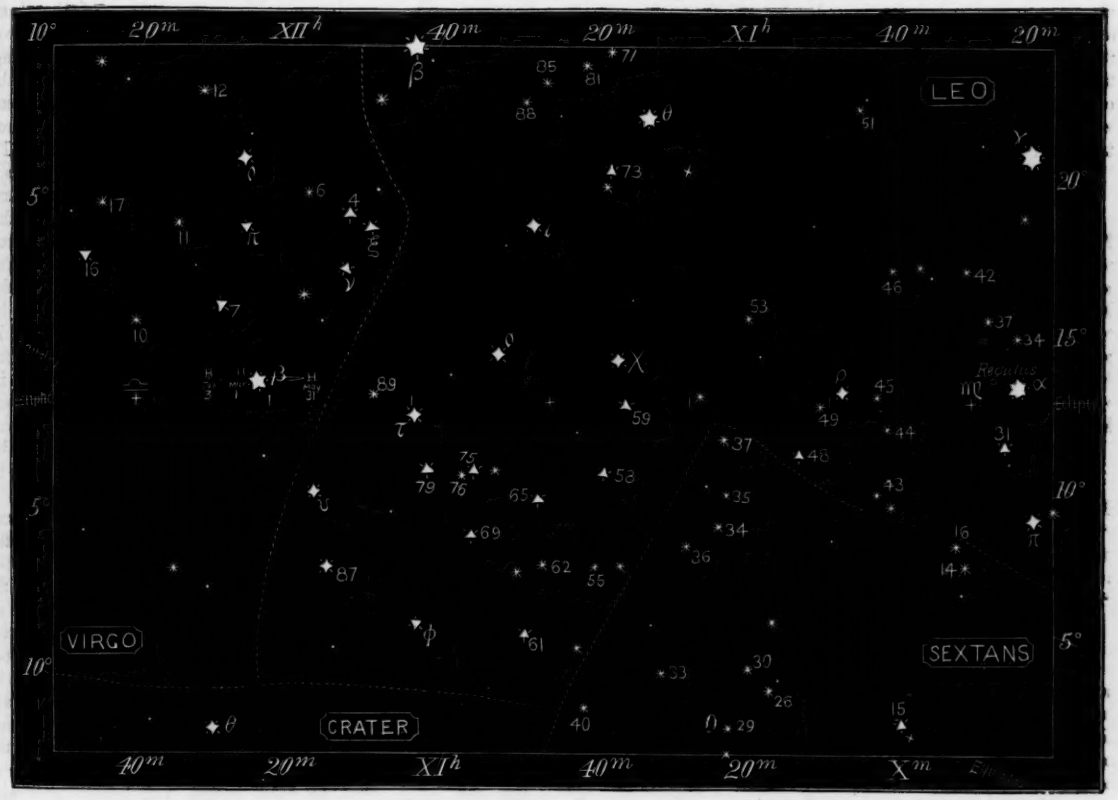
The rule applies in small matters as well as great. Mr. Spencer himself notes (though it is when dealing with selfishness specifically) a case of not infrequent occurrence, and perhaps of a trifling enough kind,—the acted falsehood of railway passengers who, by dispersed coats, make a traveller believe that all the seats in a compartment are taken when they are not. Here the detection and resistance of the attempted wrong, contemptible as it is, may excite some sense of shame in the wrong-doers, though conceivably not (for such wrong-doers are of a shameless sort); but the defeat of their purpose will at the least involve disappointment and serve as a discouragement from such attempts in the future. Of course a very zealous opponent of the obnoxious section of society might not be content with what I here advocate

* It may not be generally known outside the Roman Catholic community that the message rendered in the authorised version of the New Testament "Peace and good-will towards men," is otherwise rendered "Peace to men of good-will." The revised version reads "Peace among men in whom He is well pleased," which would in effect be nearer the Roman version.

as the simple line of duty in such cases. He might (as an earnest opponent of evil did—rather harshly I think—the other day), take on himself to punish as well as to resist evil: and having been met with the customary falsehood as to some article deposited in a vacant seat, might pitch it out of window with the remark that he would be responsible to the real owner when he appeared. But this is going beyond the strict line of duty in such matters.

It will appear manifest, I think, on careful consideration of the matter by any one who notes, for a few days or even hours, the course of events around him in his family and in society, that he who neglects to defend his own rights against the encroachments of class D as well as of class E, and of class C as well as of class D, fails as clearly in his duty to the social body, as the parent who overlooks selfish and unruly conduct in his children. And just as the children themselves whose training is thus neglected have really just reason did they but know what is good for them to complain of such mistaken kindness, so even the more selfish (all but the members of class E) have no less reason than the unselfish, did they but know their own interests, to desire that considerate but firm and self-regardful conduct should prevail throughout the body social.

(To be continued.)



THE ZODIACAL SIGN FOR FEBRUARY,

WITH THE PATH OF URANUS IN 1884.

WE give this week the sign of the Zodiac—to wit Virgo, which is now dominant in the midst of night, and the path therein of Uranus. The constellations now occupying this sign where once Virgo reigned supreme, are Leo and the head of Virgo.

ELEPHANTIASIS AND ELECTRICITY.—An interesting communication on the treatment and cure of elephantiasis amongst Arabs by Doctors Moncorvo and Silva Aranjo has been presented to the French Academy of Sciences by M. Gosselin. The cure consists in decomposing the tumid swelling of the limbs, known as elephantiasis, by means of electrolysis, but at the same the general health of the patient is also treated hydropathically, that is to say, by the cold-water cure, sea-baths, tincture of iodine, iodide of iron, arsenic, and other tonics. These medicines are intended to renovate the constitution, but are not of themselves sufficient to reduce the tumours. Electropathy, however, applied as soon as possible after the first manifestation, checks, and ultimately cures it. The cure is generally perfect, and takes place at the end of a few days in some cases; but if the elephantiasis is of long standing the cure is also a long process, and must be accompanied by proper medicines. The electrolysis is effected both by continuous and interrupted currents sent through the tumid swelling.—*Engineering.*

Gossip.

The *Spectator* considers the views recently expressed by Professor Tyndall about political matters erroneous, ("and so," by the way, "does this doggrel bard"); but is that a reason for making rude jests about that eminent man of science? That because a man has shown exceptional power in scientific research his judgment about matters outside science is not therefore necessarily sound may be true enough; but it is going rather too far to assert that therefore his judgment is necessarily unsound, and going a great deal too far too imply that therefore the man of science makes himself ridiculous by expressing an opinion at all. On the whole his opinion is at least as likely to be correct as that of the average writer of newspaper leaders. But then the public seem to think that every leading article in the daily and weekly papers is written in solemn conclave of all the staff, and that the staff includes several members of the Government, three or four generals, an admiral or two, and a fair sprinkling of bishops.

The same sort of treatment has been extended to a literary man—Mr. Sala. When he writes in a daily paper without his name appearing, readers regard what he says as the utterances of that mysterious entity, a newspaper. But when he writes over his own name the admirable weekly "Echoes" in the *Illustrated London News*, he is regarded as fair game for the foolish folk who suppose that because a man has shown marked skill in one subject he must therefore necessarily be not worth listening to when he speaks about any other.

The last polite thing said to him was that as his name has a foreign sound he is not of purely English blood, and "were my will paramount" wrote a solemn idiot, none but those of purely English blood should express an opinion about English matters,—which would be rather hard on "the queen and all the royal family."

The red afterglow that has caused so much discussion among philosophers is now explained by a correspondent of the *Scientific American*, who asserts that the phenomenon is due to the red spot from the planet Jupiter. This great rosy cloud disappeared several months ago from the atmosphere of Jupiter, has had just time, according to this correspondent, to travel to our earth, and is now hovering over us, causing the ruby colouring of our skies night and morning. Nobody ever has or will be able to prove that this is not the fact; therefore, it must be true, says the correspondent. The question is settled; it is useless to talk further about cosmic dust, Java ashes, or aqueous vapour.

It is stated, says the *Electrician*, that the *Great Eastern* has been purchased by a firm who intend to use her as a coal-hulk at Gibraltar. Perhaps it is better that she should be made use of than that her bones should rot in Milford Harbour. But what a come-down for a vessel which laid the first Atlantic cable! She has in her lifetime played many parts, and, after all, perhaps the last will not be the least useful.

A Time Corrector.—Mr. Latimer Clark, by his recent study of the transit instrument and the publication of his transit tables for 1884, has given private individuals a means of getting accurate time from the stars, though they have no acquaintance with astronomy. The transit instrument required is a very simple one, easily used, and the tables save all calculation. Another useful device to serve the same end is the "solar regulator" of M. Corneloup, a French clockmaker. This is really a portable sun-dial, which can be carried on the person and set up anywhere. A compass enables it to be placed in the true horizontal position, and a graduated sector with clamp, carried by an upright support, enables the hand or style to be set at the proper inclination, which should correspond with the latitude of the place. Noon is shown by a ray of sunlight shining through a hole in the style, and falling on the noon line drawn beneath it.—*Engineering*.

British Association for the Advancement of Science.—In accordance with a resolution of the Committee of Section A (Mathematical and Physical Science), passed at the meeting in Southport, 1883, the organising committee of the Section have selected the following subjects for special discussions during the meeting in Montreal (1884):—1. On Friday, Aug. 29: "The seat of the electromotive forces in the voltaic cell." 2. On Monday, Sept. 1: "The connection of sun-spots with terrestrial phenomena." The Organising Committee hereby invite mathematicians and physicists to co-operate with them in sustaining the discussions by contributing original papers or oral communications bearing on the selected subjects. In order that the best arrangements may be made, the names of those who propose to read papers or are willing to take part in the oral discussions should be sent not later than June 1, 1884, to the Secretaries of Section A, British Association, 22, Albemarle-street, London, W. No paper should in reading occupy more than fifteen minutes, and no speech more than ten minutes. Communications on other subjects in mathematical and physical science will as heretofore be cordially welcomed, and of these notice should be sent to the Secretaries, as above, not later than July 1, 1884.

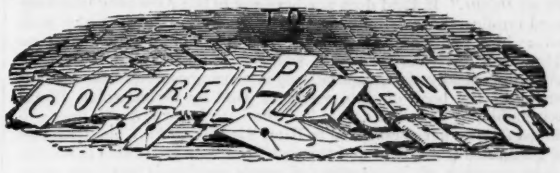
The Sun's Atmosphere.—In considering solar surroundings we must set on one side the notion—which still seems to underlie the reasonings of many on this subject—that we have ordinary atmospheric conditions to deal with. Whatever elements exist around the sun either above or below that visible surface which we call the photosphere, they cannot possibly be in such a condition as to be subject to those laws of gaseous pressure which we recognise in gases at ordinary temperatures. This may easily be shown. Let us suppose an atmosphere of some gas around the sun which at a given height, say a hundred miles below the visible surface, has one-hundredth of the density of air at the sea level, or—to take round numbers—say one-100,000th of the density of water. Then were the laws of gaseous pressure as they exist in our own air prevalent in the sun, the density of this hypothetical gas would be doubled about nine times, or increased more than 500 times a mile nearer the sun's centre, 500 times 500 times or 250,000 times a mile lower down, where it would be therefore equal to $2\frac{1}{2}$ times the density of water,—or considerably greater than the density of the sun. Now above and below the visible surface of the sun that diffused complex envelope extends for thousands of miles which we are accustomed to regard as a complex atmosphere; and it would be absurd to admit the possibility that within a range of only a few miles a density exceeding the average density of the sun's whole globe would be acquired by an atmospheric gas much rarer than hydrogen at the sea level. Either then the envelope is not in the condition which we call gaseous, or the behaviour of such gases at or near the surface of the sun is quite unlike that which they display under such conditions as we can experiment upon on this earth.—R. P.

THE FACE OF THE SKY.

FROM MARCH 14 TO MARCH 28.

BY F.R.A.S.

THE sun continues to exhibit fine individual spots and groups of spots, and should be examined with the telescope on every clear day. The zodiacal light may be looked for in the west after sunset. The night sky is delineated on Map III. of "The Stars in their Seasons." Minima of Algol (id. Map. I) occur 28 minutes after midnight on the 16th, and at 9h. 17m. p.m. on the 19th. Mercury is a morning star during the next fourteen days, but is practically invisible. He comes into superior conjunction with the sun; in other words, passes behind him at the end of the month, and is too close to him to be seen. Venus continues to increase in brilliancy as an evening star, and is a beautiful object in the western heavens. It is nearly half-past 10 o'clock at night towards the end of the next fortnight, before she sets, which she does in the NW by W point of the horizon. Mars is visible during all the working hours of the night, but is best seen between the hours of 8 and 10. His angular diameter has now perceptibly diminished, and he appears but a small object as contrasted with Jupiter. The Zodiacal Map on p. 70 shows his path in Cancer during the period covered by our notes. Jupiter may be also well seen during the greater part of the night, but should be looked at as soon after dark as possible to be caught in his most favourable position for observation. His angular diameter has also decreased since January, but the change is less apparent than in the case of his neighbour in the sky. His pendulum-like path in Gemini and Cancer is depicted in the Zodiacal Map on p. 40. The phenomena exhibited by his satellites before 1 p.m. during the next fourteen days are both numerous and interesting. To-morrow evening (the 15th) the Shadow of Satellite II. will enter on to his disc at 7h. 24m. Satellite II. itself will pass off his opposite limb at 8h. 5m., followed by the shadow at 10h. 19m. Analogous phenomena of Satellite I. will occur subsequently, but after our prescribed hour. On the 16th Satellite III. will be occulted at 7h. 47m., as will Satellite I. at 10h. 49m. Satellite III. will re-appear from occultation at 11h. 20m. p.m.; only, however, to suffer eclipse at 12h. 24m. 7s. On the 17th the transit of Satellite I. will begin at 8h. 8m. p.m.; that of its shadow at 9h. 17m. The Satellite will leave the planet at 10h. 28m.; the shadow it casts at 11h. 37m. p.m. On the 18th Satellite I. will re-appear from eclipse at 8h. 43m. 27s. p.m.; as will Satellite IV. on the 21st. at 8h. 22m. 19s. This last-named re-appearance should be looked for by every possessor of an accurate clock, whose rate and error are known, for comparison with the predicted time of its occurrence. On the 22nd Satellite II. will enter into Jupiter's face at 7h. 39m. p.m.; followed by its shadow at 10h. 1m. The egress of the satellite happens at 10h. 34m.; that of its shadow at 56m. after midnight. On the 23rd, Satellite III. will be occulted at 11h. 30m.; as will Satellite I. at 12h. 40m. p.m. On the 24th, Satellite II. will reappear from eclipse at 7h. 56m. 6s. p.m. Then the transit of Satellite I. will begin at 9h. 59m.; as will that of its shadow at 11h. 11m. The satellite quits Jupiter's opposite limb 19m. after midnight; the shadow not until half-past 1. On the 25th, Satellite I. reappears from eclipse at 10h. 38m. 49s. p.m. On the 26th, the egress of the shadow of this same satellite takes place at 8 o'clock; and, finally, on the night of the 27th, the shadow of Satellite III. passes off the face of the planet at 9h. 59m. Saturn is now rapidly approaching the west, and must be looked for as soon as ever the twilight deepens sufficiently for him to be seen. He sets about half an hour after midnight to-night, and between 11 and 12 o'clock by the 28th. Saturn continues to the west of ϵ Tauri, the upper star is the Hyades ("The Stars in their Seasons," Map I.) Uranus is now getting very close to β Virginis ("The Stars in their Seasons," Map V.). In fact, he will be only some $4\frac{1}{2}'$ north of that star on the night of the 28th. He comes into opposition to the sun at 6 a.m. on the 16th, and is visible all night long. Neptune is invisible. The moon is 16·7 days old at noon to-day; and, of course, 28·7 days old at the same hour on the 26th. Then on the 27th at noon her age is 0·3 day; and quite evidently 1·3 days 24 hours later. No occultations of fixed stars occur during the next fortnight. The moon is in Virgo to-day and to-night, but at 10 o'clock to-morrow morning (the 15th) passes into Libra. She continues in this constellation until noon on Monday, the 17th, when she enters the narrow northern prolongation of Scorpio. She takes ten hours to cross this, and then emerges in Ophiuchus. This she quits for Sagittarius at 7 p.m. on the 19th. She occupies until 8 a.m. on the 22nd in travelling through Sagittarius, at which hour she enters the north-western part of Capricornus; which she quits at midnight for Aquarius. There she remains until 6 p.m. on the 25th, when she crosses into Pisces. Her journey through this great constellation is not completed until 4 p.m. on the 28th, when she passes the boundary into Aries. She is in Aries when these notes terminate.



"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the EDITOR OF KNOWLEDGE; *all Business communications to the* PUBLISHERS, *at the Office, 74, Great Queen-street, W.C.* IF THIS IS NOT ATTENDED TO DELAYS ARISE FOR WHICH THE EDITOR IS NOT RESPONSIBLE.

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NO COMMUNICATIONS ARE ANSWERED BY POST, EVEN THOUGH STAMPED AND DIRECTED ENVELOPE BE ENCLOSED.

MR. SPENCER AND THE *EDINBURGH REVIEW*.

[1136]—You ask me to "condescend" to answer your complaint that I have attributed to Mr. Spencer "an absurd opinion about the Almighty contrary to the plain meaning of his words." I will, when you tell me what you take to be his plain meaning about the Almighty, consistent with his language throughout the book. Mr. F. Harrison has told us very plainly, indeed, in the present number of the *Nineteenth Century*, what he takes it to be, and intimates that Mr. Spencer had better not have left it so hazy—at least in language. And Mr. Harrison looks at him from a very opposite pole to mine. I referred to several passages in the "First Principles," which, with sundry others, at pages 161, 169, 170, 552, and 555, &c., prove that he attributes no action at all to his "Unknowable, Unconditioned," &c., except making or maintaining Persistence Force, which "works in us," and is "the Ultimate of Ultimates" and several more tremendous things; and also saying that it makes no practical difference whether that Unknowable made or is itself that Ultimatum; wherein I admit that he is right, if that is all that it does. Now, then, please, let us have your "plainly worded and exact description" of his plain meaning, and we shall know where we are, and what I have to answer.

AXIOMS.—I have not the least objection to your proving that the Greek philosophers did not know Greek enough to see that they had no business to use *ἀξίωμα* for only self-evident or necessary truths, not depending on experience; and that the Greek and English dictionaries are equally wrong in so translating it and its merely English form; and that what they ought to have used it for was "all truth that is worthy (*ἄξιος*) to be received": that is, all truth, and all experimental results, and all the laws of nature, old and new. But, on the whole, I have a preference for using words in their "agreed senses"; and no less a person than Professor Huxley says, in his "Hume," that it is mere beating of the air for arguers not to do so. I thoroughly agree with him in that, though I have seen him charged with conspicuously breaking his own rule in that very same book; but that is nothing. If a great man may not set up his own vocabulary and use it as long as is convenient, and then quietly slide back upon the old one, this would no longer be a free country, and a great deal of new philosophy would be nowhere. I only showed that Mr. Spencer largely availed himself of that privilege, not only as to axioms and postulates, but many other words. I cannot write a great part of the review over again here to prove any of these things, and, if I have not shown them already to those who choose to see, I should not be more successful here. THE EDINBURGH REVIEWER.

[On the first part of the Reviewer's letter, I simply note that to ask me what Mr. Spencer understands by what (with others whose opinion has been thought of some weight) he admits to be not understandable, is a convenient way of putting off an act of justice till the Greek Kalends. Mr. Spencer expresses in his own way (in the words the Edinburgh Reviewer finds so tremendous) very much what the Apostle of the Gentiles expressed in his, when he said "How *unsearchable* are God's judgments and His ways *past finding out!* For *who hath known* the mind of the Lord? or who hath been

his counsellor? Or *who hath first given to Him, and it shall be recompensed to Him again?* For *of Him and through Him and to Him are all things.*" If Paul does not here say of the Almighty (the very word implies the attribute of Persistent Force) what Elihu said before him, and what Mr. Spencer is so blamed and ridiculed by an Edinburgh Reviewer for saying, words have no meaning. Distinctly and emphatically the comparatively modern apostle and the ancient writer of the book of Job, speak of God as Unknowable ("unsearchable," "past finding out," "who hath known, &c."), unconditioned ("Who hath first given to Him, His ways," &c.), the Ultimate of Ultimates ("Of Him and through Him," &c.). If this is not the plain meaning of Mr. Herbert Spencer, neither is it of the older writers. The Edinburgh Reviewer should have begun his unfair attacks earlier and directed them elsewhere.

With regard to the word Axiom, the Edinburgh Reviewer conveniently begs the whole question, and (as conveniently) overlooks what I originally said—that Newton used the word axiom in the sense of "a fact or law established by experience, and (so) known to be worthy of acceptance,"—as distinguished of course from facts or laws established by reasoning. This includes those axioms which the careless regard as the only kind of axioms,—self-evident propositions; for their self-evidence arises only from the commonness of the experience. It also includes the meaning which some careless makers of dictionaries have omitted to notice,—those laws or general principles which though not self-evident have been established by experience, and do not admit of being demonstrated, but must be used as the basis of demonstrations. They might be defined as "laws which have to be admitted without proof," and explained as "laws which are either self-evident or established by universal experience." No one that I have ever heard of except the Edinburgh Reviewer has ever thought of regarding the Laws of Motion as self-evident.

Here follow a few extracts which may interest an Edinburgh Reviewer:—

AXIOM: a word derived from the Greek ἀξίωμα, which is formed from the Greek verb ἀξίοω, *to think worthy of* and hence to *desire* or *demand*. " . . . ." It is usual to define an axiom *as a self-evident proposition*; but this . . . . is not a good definition." . . . . "A self-evident proposition as such ought not to be called an axiom." —De Morgan in the *Penny Cyclopædia*.

AXIOMS: the fundamental elements or acceptances of science . . . . *Propositions that result from universal experience* in the elementary physical sciences, such as the sciences of motion and force must be accepted as axioms.—Author of the article Axioms (Qy. Sir W. Thomson) in Nichol's "Cyclopædia of the Physical Sciences."

ἀξίωμα, That which is thought fit; a decision.—Liddell & Scott.

AXIOMS.—"The general principles which are necessary to knowledge are *axioms*.—Aristotle, Anal. Post, i. 2. "It is the office of *observation* to supply principles in each subject." Aristotle, Anal. Prior., i. 30.

AXIOMS.—"Aristotle and other ancient philosophers not only asserted in the most pointed manner that all our knowledge must begin from experience, but also stated in language much resembling the habitual language of the most modern schools of philosophy, that particular facts must be *collected*; that from these general principles must be obtained by *induction*; and that these principles, when of the most general kind, are *axioms*."—Dr. Whewell's "History of the Inductive Sciences," vol. i., p. 75.

INNATE PRINCIPLES, AXIOMS, OR MAXIMS.—"It is an established principle amongst some men, that there are in the understanding certain innate principles, some primary notions, κοιναὶ ἔννοιαι, characters, as it were, stamped upon the mind of man which the soul receives in its very first being, and brings into the world with it: it would be sufficient to convince unprejudiced readers of the falsehood of this supposition if I should only show how men, barely by the use of their natural faculties, may attain to all the knowledge they have, without the help of any innate principles, and may arrive at certainty, without any such original notions or principles" —with much more showing how axioms or maxims are established by experience—"discovered by the application of those faculties that were fitted by nature to receive and judge of them when duly employed about them."—Locke on "The Understanding."

AXIOM.—"An established principle in some art or science which, though not a necessary truth, is generally received."—Webster's "Dictionary."

SIR ISAAC NEWTON ON THE AXIOMS OR LAWS OF MOTION. "Hitherto I have laid down *such principles as have been received by mathematicians and are confirmed by abundance of experiments.* By the two first laws and the two first corollaries, Galileo discovered that the descent of bodies observed the duplicate ratio of the time, and that the motion of projectiles was in the curve of a parabola, *experience agreeing with both*, unless so far as these motions are a little retarded by the resistance of the air. On the same laws and corollaries depend those things which have been demonstrated concerning the times of the vibration of pendulums, and are *confirmed by daily experiments* of pendulum clocks." Then follow five or six pages of experiments by which Newton himself, following a line of experiment "begun by Sir Christopher Wren, Dr. Wallace, and Mr. Huygens, the greatest geometers of our times," established the third law. "And thus," he says, after the first part of his series of experiments, "the third law, so far as it regards percussions and reflexions, is proved by *a theory exactly agreeing with experience.*" "In attractions," he proceeds, "I briefly demonstrate the thing after this manner";—then follow the experiments and reasonings. He concludes with the remark that he wished "to show *by those examples* the great extent and certainty of the third law of motion."

This shows, (what indeed is self-evident) that Newton did not regard the laws of motion as self-evident propositions. The extracts preceding the last show that he was nevertheless perfectly justified as well by the ancient as by the modern use of the word, in calling them *axioms*.

I do not hold myself bound to give an Edinburgh Reviewer further opportunities to repeat (which is all he does) his unfair and misleading attacks on Mr. Spencer. I was moved by his injustice to the comments I made, which he has not met in any way. I believe the stroke of the publishers in advertising Mr. Spencer's work with the Edinburgh Reviewer's most acrimonious sentence appended as if it were a laudatory criticism, was almost as good a way of responding as the case admitted. The review however homicidal in intent proved but suicidal in effect: it has gone far to destroy the attack it was intended to sustain.

RICHARD A. PROCTOR.

"THE NEW PRINCIPIA."

[1137]—In your impression of to-day you assert that I cannot see this, that, and the other, all of which I do see very plainly; and in reply to one set of arguments you deal out a quantity of irrelevant elementary information which is to be found in every text-book on the subject. I am well acquainted with Sir Edmund Beckett's "Astronomy without Mathematics," but as far as my ideas are concerned, it is a work quite beside the mark. Grant Sir Isaac Newton's premisses and his conclusions are irresistible; but what I dispute is not his demonstrations but his assumptions. For instance, when the earth's motion is accelerated in perihelion, Newton *assumes* [!!, R. P.] that the increased speed developes an increased centrifugal tendency which "grows as the square of the velocity." This would be the fact in relation to two bodies mechanically united, one compelled to revolve round the other; but I contend that as an explanation of the movements of the planets, which are not mechanically attached, it is an arbitrary surmise and an "impossible" supposition. What Newton mistook for centrifugal force, under the circumstances, is in fact, according to my theory, a phenomenon of magnetic repulsion.

You tell us that your system is not simple, and that it cannot be understood except by mathematicians. On the other hand, I claim for my system that it enjoys the advantages of simplicity, and of being intelligible to any educated mind of average ability.

I have shown in "The New Principia" that certain phenomena—viz., the extra labour of ascending a mountain, the behaviour of comets and asteroids, the tides, and the planetary notions generally —are not reconcilable with Newton's hypothesis of gravitation; but that they are beautifully confirmatory of the theory of Polarity. Nothing that you or any one else has said has in the slightest degree succeeded in shaking my position. On the contrary, you have effectually shown that your doctrines are complicated, untenable, and visionary to the last degree. Instead of making your theory distinctly, simply, and incontestably, explanatory of your facts, you are obliged to trim, twist, and qualify your theory to suit the facts.

London, *March* 7, 1884. NEWTON CROSLAND.

[Mr. Crosland must excuse me if I fail further to deal with his notions. Readers complain already and with some justice. The subject he has attacked is simply outside his range. It is rather hard on him perhaps to publish the above letter, as it so clearly shows how utterly astray he is. But in justice to myself it must appear. I have done my best for him, and must now in all courtesy withdraw.—R. P.]

THE FELLOWSHIP OF THE LEARNED SOCIETIES.

[1138]—I have been reading with much interest the remarks by "N. W." in your issue, number 119. They are, as you say in your foot-note, "very sweeping," but I certainly think they are very true. It is a subject which has afforded me some thought lately, and I am very glad to see others take it up. It is sad indeed to see these societies in such a bad condition. They are old, most of

them, and have been honoured greatly by some names which they have enrolled in their lists, and it is a great pity to admit such people as "N. W." refers to. Surely there are still some Fellows left who have pride in their society, and would try to prevent this growing evil. I would propose that it could be remedied by appointing, say, three members of the council to hold periodical examinations, either by paper or *viva voce*, and for candidates only who have passed this examination to be eligible for a Fellowship. It would not involve very much trouble; half-a-dozen searching questions would be a sufficient test, and would soon work a reformation in the class of men who would be admitted.

I hope "N. W." will see this letter, and, if he is a Fellow of any of these societies, that he will endeavour to carry out this plan, or any other that he may conceive which would bring about the same result.

The subject is one on which many people feel strongly; and, if either the letter of "N. W." or this one has the effect of stirring up some Fellows to action, we shall both feel gratified in thinking that our words have brought forth some good fruit.

S. DE C. THOMPSON.

THE GREAT PYRAMID.

[1139]—In your book on the Great Pyramid you said that the probable date of its erection was at or near 3,400 B.C. I was reading at the same time a book on the ancient history of the East (I forget the name of the author), and it gave the dates of the different dynasties, according to Manetho. I believe the earliest Egyptian date that is really known is the Expulsion of the Shepherds in the year 1703 B.C.

Manetho gives, if my notes are correct, the following periods for the different dynasties:—

For the 1st, 253 years
" 2nd, 302 "
" 3rd, 214 "
" 4th, 284 "
" 5th, 248 "
" 6th, 203 "
For the 7th to 11th, uncertain interval computed at 436 years
For the 11th and 12th, 213 years
13th, 453 "
14th, 184 "
Shepherds, 511 "
Ending at 1703 B.C.

As the Great Pyramid was built at the beginning of the 4th Dynasty, these figures, if added together, would bring the date to about 4200 B.C.; but, probably, the 13th and 14th Dynasties were reigning in what we call Central Egypt, while the Shepherds were reigning in the Delta. According to my idea, the dates would be something like the following:—

		Length of reign.
Accession of 4th D.	3435 B.C.	284
Great Pyramid built	3403 B.C.	
Accession of 5th D.	3116 B.C.	248
" 6th D.	2868 B.C.	203
Interval (probably overrun by barbarians)	2665 B.C.	112
Accession of 11th D.	2553 B.C.	213
" 12th D.	2340 B.C.	453
Invasion of Shepherds	2214 B.C.	511
Accession of 13th D.	1887 B.C.	184
" 14th D.	1703 B.C.	

The 14th D., beginning at the expulsion of the Shepherds. Have any dates really been fixed for these events? MENES.

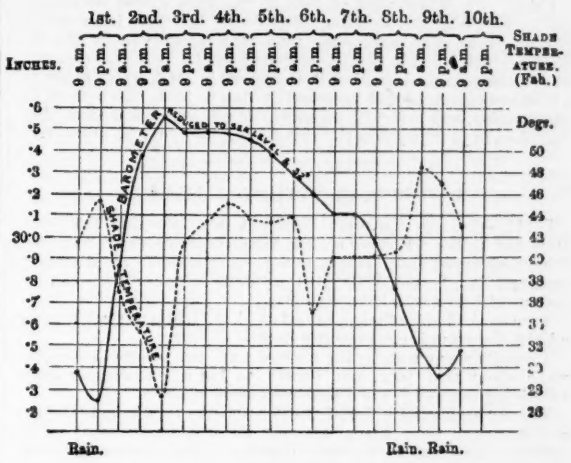
[Dates have been given—the trouble is that so many have been suggested.—R. P.]

BAROMETER CURVES AND A SPECTROSCOPIC EXAMINATION OF SUNRISES AND SUNSETS.

[1140]—I have often been struck with the symmetry of barometrical curves, although from want of sufficient mathematical knowledge I am unable to investigate their properties. The curves to which I allude are formed by the union of points on a scaled diagram taken at intervals of twelve hours.

On looking at my meteorological diagrams I find one such curve from July 1st to 13th last year, a period of fine dry weather, broken only by thunderstorms on the 9th and 10th. But a still more remarkable example is found from Feb. 1st to 9th of this year. I give a draught of this curve, as well as of the curve of shade temperature, taken at the same hours of observation.

DAYS OF THE MONTH OF FEBRUARY, 1884.



The barometer readings are taken by a standard instrument verified at Kew, and they have been reduced to sea-level and 32 deg. The shade temperature (with Kew corrections) is taken in a Stevenson screen.

A short time ago one of your correspondents, I think, suggested the spectroscopical examination of "fore" and "afterglows" at sunrise and sunset. I have accordingly observed them carefully for the past month or so. I have used for this purpose one of Browning's microspectroscopes, measuring the lines and bands with the attached micrometer, having previously ascertained the positions of the solar lines, and set them down on a map. The result I have compared with Angstrom's map of the spectrum measured in wave lengths, especially with those parts of the spectrum which are marked with the variable telluric (absorption) lines and bands. I enclose a diagram of the spectrum so marked.

As an illustration, I may take the beautiful sunrise which occurred here on the 13th of this month. At 7 a.m. there was a dark, blood-red strip close to the horizon, and dark clouds above. At 7.15, yellow rays spread over the sky as if the sun were first about to make his appearance. At 7.25, I again looked out, and what was my astonishment to find the sky lit up with an intense red, crimson near the horizon and pink near the zenith.

On looking through the spectroscope at an angle of about 10° elevation, I noticed a nearly total obscuration of the violet end of the spectrum—the dark green was very faint; but the red end was very brilliant. I could just see the line *a*: B was broadened out to a thick black stroke; C was enveloped in an aqueous vapour band. The line marked *a* was very pronounced and dark. But by far the most prominent of the absorption bands was the broad shading marked *δ*. This seems to be the most characteristic band of the whole spectrum, and occurring, as it does, near the lightest part of

the spectrum, is seen earliest in the day and latest in the evening. There was no very considerable vapour-band to the immediate left of D, and I could not detect any absorption at *b*, or to the left of F.

Whatever may be the cause of the brilliant colours at sunrise and sunset, it seems clear that they do not arise from *excess o*

vapour; for the most characteristic bands at B, *a*, and *δ* are not vapour-bands, but dry air-bands, and appear to be due to the presence of some gases or particles of other bodies floating in the air, such as meteoric dust, &c.

Perhaps your correspondents may be able to throw some light on this interesting question. HENRY J. POOLE.

Feb. 15, 1884.

INVENTION OF THE TELEPHONE.

[1141]—In KNOWLEDGE of Feb. 29 you have a most interesting article on "Who Invented the Telephone?" but there is a mention of a telephone at an earlier date than that referred to by Mr. W. Slingo, which I find in a small book on electricity by R. M. Ferguson, Ph.D., and published by W. & R. Chambers in 1867, which refers to a telephone invented and patented by Reis in 1861, which, from its construction, was evidently an earlier-dated conception than that described in your paper of the above date. W. S. JEFFERY.

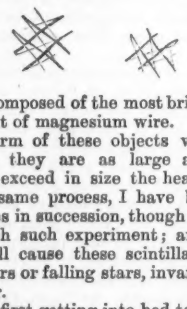
A SINGULAR OPTICAL ILLUSION.

[1142]—During the past fortnight my attention has been attracted to the following curious visual experiences; and though I have observed them before, and possibly mine is not a solitary instance, yet I should be glad to know whether they can be accounted for on scientific principles.

I am in the habit of going to bed at 11 to 11.30 p.m.; not burning a light, the room is consequently in total darkness.

I usually sleep soundly till about 4 a.m., but almost invariably wake about that hour, and remain so for a considerable time, remaining more or less in a semi-recumbent position.

I found accidentally that if I close my eyes for a minute or two, and then open them again, throwing the head back slightly at the same moment, that I see exactly opposite to, and apparently within six feet of me, two figures, thus—



the dark lines being composed of the most brilliant streaks of white light, as bright as a bit of magnesium wire.

Occasionally the form of these objects varies, and their size also; for sometimes they are as large as a sixpenny piece, and at others do not exceed in size the head of an ordinary pin. Some nights, by this same process, I have been able to reproduce this effect several times in succession, though a short interval seems necessary between each such experiment; and, again, some nights no efforts of mine will cause these scintillations to appear—they look almost like meteors or falling stars, invariably seeming to drop down and so disappear.

I have also tried on first getting into bed to see if I could induce them, but without success; they never come till I have had at least a couple of hours' sleep.

I ought, perhaps, to add that for ordinary purposes the sight of my eyes is unequal; the focus of the right is much shorter than that of the left eye, any object or printed matter appears *at least* one-third smaller when looked at with the right eye, yet these luminous lines and stars always seem the same size to both eyes.

I send this, hoping that some of your correspondents may be able to tell me whether these optical illusions arise from general causes, or from some peculiar nervous action of the retina.

COSMOPOLITAN.

A STRANGE DREAM.

[1143]—A curious instance of a dream coming true occurred to me this winter.

While at college last term I had the following dream:—I dreamt I was going to visit a strange place, and I was permitted to take a friend with me. I took a fellow named A., and we started for the visit.

It was a pouring wet day, and we were compelled to take a cab from the station. We drove several miles before we reached the house, and when we did so the friend had gone.

I rang the bell and a servant showed me into a room—I supposed the drawing-room. On my left on entering was a fire-place; a lady was working by it. I could not recognise her features. A window was opposite, through which I saw a lovely landscape, though spoilt by the rain; a young man was in the window, and, as I entered, he was saying, "Bother the rain; I meant to have gone to meet H." (myself). I noticed his face particularly. I also noticed other articles of furniture in the room.

With that my dream ended. I was so struck with it that I related it fully to A., and also to another friend, next morning.

I forgot all about it until the vacation.

I was staying with A., when I had a letter from some distant relations, up to now unknown by me, inviting me to go and stay with them while I was in the neighbourhood. I accepted, and the day on which I left A.'s house was a "drenching" one.

I reached the station of E——, and as it was so wet, I thought no one would meet me, so I took a cab. C—— was five miles from the station. I reached the house at length, and was shown into the drawing-room.

The lady of the house was working by the fire on my left, as I entered; and one of my cousins was standing at the window, and I heard him saying, before he saw me, "I wish H. would come. Bother the rain; I meant to have gone to meet him." He turned towards me as I went in, and I recognised at once the face in my dream.

The view was exactly the view I had seen before, and the details of furniture were placed as dreamed. I had never seen the people before, nor even heard of them, and had never been within fifty miles of the house.

I was much struck with the circumstance, and should like to know any good theory as regards prophetic dreams.

H. D. HINDE.

STRANGE DREAMS (ABSTRACT).

[1144]—Your article on "Ghosts and Goblins" in KNOWLEDGE of the 15th inst. reminds me of a dream I had some years ago. An old aunt of mine, of whose illness I had not heard (nor of herself for two months) appeared to me, after I had been some time asleep, in her usual attire, with a little basket of fruit in her hand, smiling and beckoning to me, exclaiming at the same time "Come along, my dear; come along," the words sounding as if really spoken by a human tongue. I awoke and looked at my watch; it was about four minutes past 12 o'clock. The background was so bright that it threw the figure of the body out, so that I could discern even the wrinkles in the face. For a time, I could hardly rid my mind of the thought that she had really appeared in person. But, regarding it as only a dream, I determined to inquire after her next day. Next morning I chanced to meet a person who knew her. Before I had spoken a word, he said, "You know old P— H—? She's dead." "Dead!" I said. "Why, I dreamt of her last night. At what time did she die?" "As near as possible, three minutes past 12." I then related the dream.

I have since had two or three such dreams. Last December, a person whom I knew very well was ill, and on the previous afternoon I inquired of a friend how he was getting on. He told me the doctors said that they had done all they could for him. That night, or rather next morning, I saw a coffin at the bottom or end of my bed, with his name and date of death upon the plate. Meeting my friend in the morning, I said, "Then So-and-so is dead," mentioning the name. He said, "How did you know that?" I said, "He died this morning, didn't he?" He said, "I have just had a telegram to say so." I then told him the dream. I can partly attribute the dream to my having heard "the doctors had done all they could for him." This might have impressed my mind; but still the coincidence is singular, as I was in excellent health at the time. I am not at all superstitious, neither do I believe in ghosts.

WALTER HOLLEBON.

ON CURIOUS STONES FOUND IN SWITZERLAND.

[1145]—Chancing to have to refer to a former number of KNOWLEDGE, I saw it mentioned therein that the Geneva correspondent of the *Times* says that certain curiously-shaped stones, covered with dots, lines, circles, and half-circles are not unfrequently found in parts of Switzerland.

During this last summer, when travelling in Scandinavia, my attention was directed to stones somewhat similar in form and markings to those which he describes. I am most anxious to procure drawings of these latter in order to compare them with those in Scandinavia, and others which I have heard of as existing in the North of Europe. Can any of your correspondents or of your readers tell me where any account or description of them is to be found? COSMOPOLITAN.

STRANGE PHENOMENON—FALSE SUN.

[1146]—On the 20th Jan. I was strikingly reminded of a report of a sunset a few weeks ago when the shadows of two gentlemen were thrown by the sun's rays on a cloud of fog or mist.

It was about 8 A.M. on the 20th January, 1884, when, leaving the station at Eston, I walked through the works of Messrs. Bolckow, Vaughan, & Co. On passing the entrance a large engine was emit-

ting clouds of steam, which the wind blew across my path, and when sufficient bodies of it passed I saw the shadow of a man at work—a very giant. I stood and watched for a few minutes, and saw at about forty yards from me some slag running into the vessel used for carrying it away. Exactly in a line with myself and about ten yards nearer was a man at work with a shovel engaged (I think) in opening the beds for the reception of the iron "pigs." Then, between the man and myself, the sheets of steam were forming a screen upon which the figure of a man and his actions at his work were clearly depicted.

Being a passenger on the North-Eastern Railway between Redcar and Middlesbrough on the 24th ult. my attention was called (about 8.35 A.M.) to a curious phenomenon. The sun was just above the Cleveland Hills, and, although not brilliant, on account of some fog, it was distinct, and of a deep red colour. At a short distance from it, on the left, I could distinctly see a second sun (or the sun's shadow) not quite so large or so distinct as the real sun, but still distinct enough to attract the attention of two gentlemen, who pointed it out to me. J. E.

STRANGE PHENOMENON.

[1147]—Whilst walking in the country last Monday evening, at about 10.20, I saw the following strange phenomenon. The night was dark and calm, and a drizzling misty rain was falling, when suddenly the whole heavens became illuminated for about two seconds, and then all was darkness again. The light was not vivid, but soft, and bright enough to show clearly the surrounding objects.

If any readers of KNOWLEDGE will give me an explantion of this curious phenomenon I shall be much obliged.—Yours faithfully,

J. ROGERS.

MOCK SUNS AND MOCK MOONS.

[1148]—Thos. H. Amyot, in No. 1128, does not say whether the window from which he observed the figured repetitions of the setting sun was open or shut. If shut, and the "false suns" were seen through the glass, they may have been due to reflection from the inner surfaces of the glass.

About 25 years ago I was summoned from my lofty abode in the Midland Institute by a very enthusiastic observer, to bear witness to his discovery of a double moon, then visible through his window. He was very angry when I suggested the above explanation, but ultimately forgave me on making another discovery, viz., that the moon became single on opening the window.

I have recently made a great astronomical discovery, viz., that Venus has a satellite, subject to remarkable variations of distance from its primary. Any careful observer may confirm this discovery by viewing Venus through a window-pane. The distance between planet and satellite will vary with the thickness of the glass. W. MATTIEU WILLIAMS.

LETTERS RECEIVED.

L. H. RUDD. Thanks. Your interesting letter marked for insertion.—M. B. ALDER. Thanks for cuttings. I will not criticise, beyond saying that I envy you the confidence with which you "hold your views as truth." And yet I do not envy you. For such confidence is never found save with half knowledge. — JENNIE GRAY. "The public are" is, I think, more frequently met with; but I can give no opinion "as an authority."—G. L. D. P. I feel the utmost confidence in recommending the dearer of the two, not because it is dearer, but because I know that the difference of price is more than counterbalanced by difference in value.—A. G. Thanks for cutting from the Gloucester *Citizen*. As you say, it is unmannerly. The gentleman who arranges my lectures has explained to me how and why the *Citizen* was moved. They wanted to add 50 per cent. to their advertising price because the advertisements were *not* trade-advertisements. Regarding this as decidedly wrong, he declined to advertise at all. The *Citizen* seeks to pay *me* off for this loss of business, by declining to insert my explanation of a change in the order of lecturing, on the plea that the lectures were a trade matter. Paltry,—and innocuous.—THE DEVIL'S ADVOCATE. Nay, I spoke of those accessories only as belonging to ghost stories, not to ghosts. Science has not rejected the phenomena of which you speak as impossible. On the contrary, some of them, as the effects "produced by what is called mesmerism" are accepted as real enough. It seems to me science takes a very fair and sensible position in such matters.—W. POOLE. Vacuum would make gravity harder yet to explain. What is "the power of vacuum"?—RICH. HILTON. Your instrument ought unquestionably to show what you fail to see with it. I believe F.R.A.S. has an instrument by the same maker and of the same aperture,—which he reduces to 3 in. in making the observations for determining what such a telescope should stand.—WELL-WISHER AND CONSTANT SUBSCRIBER. Thanks for your kindly letter. There have been some who have regarded the insertion of matter not interesting to them as a personal offence. But I doubt if there are ten persons in England who take interest in *everything* in any week's KNOWLEDGE. Will "listen to Thomas Foster" and "take care of self." Thanks.—J. S. S. Very neat; *but* the moon was not between Venus and *the sun*.—A COMMON LABOURER. Good gracious what an outburst! So, I set myself up as an idol. (Your letter must have upset me, for punning indicates mental disturbance, and the feeble remark occurs to me, "I'm too busy to be an idol, man." But let that pass.) You tell me that as there is less earth under the pole than under an equatorial place, gravity ought to be less at the pole; yet it is not: wherefore you think science all wrong: and the real reason why a body falls more quickly at the pole is that the air there is less dense. Now if I say—as I do—that you are mistaken, you will twit me with idolness, and "decline to worship" (so distressing to an idol, you know). Yet it is not I but science of which I am but a servant which denies utterly the validity of what you regard as reasoning. I feel rather sorry that what you call "my impartiality in attacking the theory of the great Laplace" has pleased you. To say the truth I attacked rather those who discuss as a theory what Laplace himself suggested as a speculation only. However, I hope you may still continue to describe yourself as "one time irascible, but now penitent"—for what is "*ira*" but "*furor brevis?*" That is a recondite quotation! Here is another, which only the widest reading could have instilled into me, *Sera nunquam est ad bonos mores via*.—W. H. HUMSEN. Thanks for your interesting letter. Rather glad (so mean are we) to hear you have a flat-earth "professor" in Maryland. I fancy we *do* lose a subscriber or two by the replies you refer to. But it is our duty to flatten some of them, and we try to do it. Yes; F.R.A.S. of KNOWLEDGE is the F.R.A.S. of the *E. M.* I do not think I have ever been unfair to the *E. M.* But my having written a good deal for it would be no reason one way or the other. I ceased writing for the very reason that paradoxists were allowed an undue amount of space. They are good fun; but one may have too much of a good thing.—LONDON CLAY. Understand about star maps. Will give shortly a map of a tolerably large equatorial region—the lively Africa—properly laid down. T. Foster angry with those correspondents! My dear sir, his note is calmness itself. I have seen the letters he referred to, and most certainly I should have spoken about them much more strongly had they been written for *my* benefit. —GAMMA. The "g" is silent in "physiognomy" and sounded in "agnostic." I do not know of any definite law; but fancy, from the few words which occur to me that when the accent falls on the vowel preceding the "g" the letter is silent.—J. MCMASTER. Mars has two moons.—W. H. MOLTON. Thanks.—W. H. FRANCE. Fear the subject of cremation is hardly yet held attractive.—J. E. GORE. All the variable stars in my Library Atlas were taken from a list kindly supplied me by Mr. Baxendell, of Manchester.—J. MABE. May be; but may be not. I think Mr. Cook is nearer the truth. It has been proved unsafe to interpret in scientific way such passages as you refer to. Even if you were right people would say, What a pity the account had not been rightly understood before.—D. B. CAZAUX. Those were very strange dreams: but "what do they prove?"—Y. Z. Thanks. Will consider about the easy articles. The dotted lines in map (KNOWLEDGE, February 8th) are simply the outlines of constellations.—H. MOODY. Certainly moon could be seen at midday in March?—J. O. LINDSAY. Many thanks.—A. T. SMITH. One convex eye-lens will *do*, but not nearly so well as a properly combined pair, making an eye-piece,—see any treatise on optics.—READER OF KNOWLEDGE [(?), anonymous, disguised hand]. Evidently something has troubled you very much; but letters like yours make the kindly and courteous ones even pleasanter than they otherwise would be,—take off the sameness, as it were.—G. A. OLLARD.—M. FEW (you seem to be one of many). —L. D. SPENCER, Jun.—T. S. ETHERINGTON.—E. GAMGEE.—Glad you are pleased. Thanks for kindly letters.—SAM. Do you recall why they killed the pig? We must consider many not one or two here and there. As for your winking at the Tricycle articles and winking at *me*, you have emphatically hit the pigeon and missed the crow.—ALG. BRAY. Yes: your solution of the cribbage problem gives more than the original propounder's. Four correspondents (counting you) have sent 78 as the solution; and it cannot I think be beaten.—R. BRYANT. Fear it would only lead to more of the New Principia and many have raised strong objections.—J. EDWARDS. A purely atmospheric effect.—E. PHILLIPS. With pleasure.—J. F. CLARKE. Such large sums that repeatedly the idea of giving the matter up arose. Better go elsewhere,—ill-mannered letters do not help.

ERRATA.—In last week's Editorial "Gossip" (paragraph 2, last line but one), for "readjust" read "recognise"; and, in paragraph 6, line 27, for "light" read "life." In "Letters Received" (eleventh line from end), for "*iter*" read "*ibis*."

Our Mathematical Column.

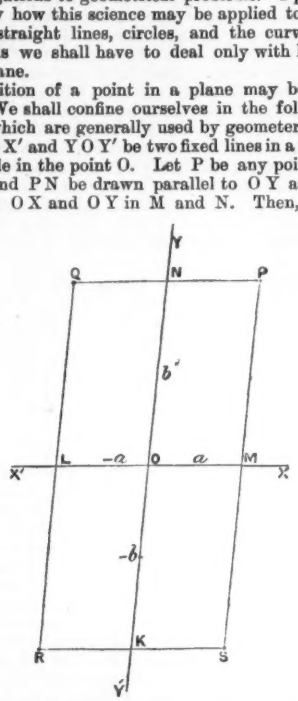
EASY LESSONS IN CO-ORDINATE GEOMETRY.

By RICHARD A. PROCTOR.

(1) Co-ordinate Geometry is that science which applies algebraical investigations to geometrical problems. I propose in these lessons to show how this science may be applied to investigate the properties of straight lines, circles, and the curves called Conic Sections. Thus we shall have to deal only with lines and curves lying in one plane.

(2) The position of a point in a plane may be determined in many ways. We shall confine ourselves in the following pages to two methods which are generally used by geometers:—

(I.) Let X O X′ and Y O Y′ be two fixed lines in a plane, intersecting at any angle in the point O. Let P be any point in the plane; and let P M and P N be drawn parallel to O Y and O X respectively, meeting O X and O Y in M and N. Then, if the lengths



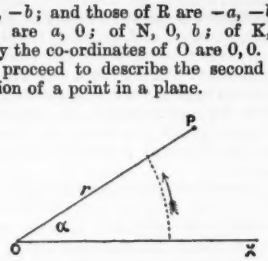
of the lines O M and O N are known, we can determine the position of P. For we have only to measure off O M and O N along O X and O Y, and through the points M and N to draw M P and N P parallel to O Y and O X respectively; the position of P is determined by the intersection of the lines M P and N P.

This method of determining the position of a point in a plane is called the *method of rectilinear co-ordinates*; X O X′ and Y O Y′ are called the *axes of co-ordinates*, and the point O the *origin*. P M and P N are called the co-ordinates of P. It is evident that we might have determined the position of P by *cutting off* O M from O X, and drawing the *parallel* M P of the required length, that is, equal to O N. Hence O M is often called the *abscissa*, and M P the *ordinate* of the point P. O X is called the *axis of x*, and O Y the *axis of y*, and distances measured along O X are generally denoted by x, distances measured along O Y by y. Thus if $OM = a$, and $ON = b$ we should express this by saying that "for the point P, $x = a$, and $y = b$," or "the co-ordinates of P are a and b," or, briefly, we may call P the point (a, b)."

(3) If the angle X O Y is a right angle the axes are said to be *rectangular*; if X O Y is not a right angle they are said to be *oblique*. The angle X O Y is called the angle of ordination, and is usually denoted by ω. Rectangular axes are more convenient than oblique axes in general, and are, therefore, most frequently employed. Occasionally, however, oblique axes may be used with advantage. In the following pages the axes are always supposed rectangular, unless the contrary is expressly stated.

(4) In our figure we have measured $OM = a$ *to the right* of O, and $ON = b$ *above* O. If we measure $OL = a$ *to the left* of O, along O X′, and $OK = b$ *below* O, along O Y′, and draw the parallels P M S, Q L R, P N Q and R K S we shall obtain four points P, Q, R, and S, each of which might be called the point (a, b). As it would obviously be inconvenient to be in doubt which of four points should be taken in this or similar cases, the following conventions as to sign are used to distinguish four such points from each other; (i.) lines measured along O X are considered positive, and lines measured along O X′ negative; (ii.) lines measured along O Y are considered positive, and lines measured along O Y′ negative. With these conventions the co-ordinates of Q are $-a$, b; those of S are a, $-b$; and those of R are $-a$, $-b$. Again, the co-ordinates of M are a, 0; of N, 0, b; of K, $-a$, 0; and of L, 0, $-b$. Finally the co-ordinates of O are 0, 0.

(5) (II.) We proceed to describe the second method of determining the position of a point in a plane.



Let O X be a fixed line, and O a fixed point in it. Let P be any point in the plane; join P O. Then if the angle P O X be known, and also the length of O P, we can determine the position of the point P. For we have only to draw O P making the required angle with O X, and to take O P of the required length; thus the position of P is determined.

This method of determining the position of a point in a plane is called the *method of polar co-ordinates*; O X is called the *initial line*, and the point O the *pole*; P O and the angle X O P are called the *polar co-ordinates* of P. It is evident that we might have determined the position of P by supposing a line of length P O to be *carried* from coincidence with O X *through* the angle X O P. Hence P O is often called the *radius vector*, and the angle X O P the *vectorial angle*. The radius vector is generally denoted by r, and the vectorial angle by θ. Thus if $OP = a$, and the angle $POX = \alpha$, we should express this by saying that "for the point P, $r = a$, and $\theta = \alpha$," or "the polar co-ordinates of P are a and α," or, briefly, we may call P the point (r, α).

(6) Conventions as to sign similar to those described in Art. 4, are applied to polar co-ordinates; (i.) angles measured in the direction indicated by the arrow in our figure are considered positive, and angles measured in the opposite direction negative; (ii.) lines measured from O, along O P, the revolving line are considered positive, and lines measured from O, along O P produced backwards through O are considered negative. With these conventions the point P might be determined by the co-ordinates r, $\alpha - 2\pi$, or $-r$, $\alpha - \pi$, as well as by the co-ordinates r, α.

Note.—We shall throughout these papers consider the positive direction for angle measurement to be that here indicated, that is contrary to the motion of the hands of a watch.

EASY RIDERS ON EUCLID'S FIRST BOOK.

With Suggestions for Solution.

PROP. 1.

1. On a given straight line describe an isosceles triangle having each of the sides equal to a given straight line.

PROP. 2.

2. Show that there are in general eight different cases in the solution of this problem; and without drawing in the complete figure for each case show where the different lines will fall.

It will be found that if the given point be connected with either extremity of the given line, or if the equilateral triangle be described on either side of the line thus drawn, or if those sides of the equilateral triangle which pass through the given point be produced either way, a solution results.

3. If the diameter of the smaller circle is the radius of the larger, show that the given point and the vertex of the constructed triangle lie on the circumference of the smaller circle.

PROP. 3.

4. Having drawn two unequal lines, go through the complete construction involved in the method of Proposition 3; shewing that in this construction five circles appear, and that there are two pairs of equal circles.

PROP. 4.

5. If two straight lines bisect each other at right angles, any point in either is equidistant from the extremities of the other.

6. Apply the method of superposition to establish the first case of Prop. 26.

7. The line which bisects the vertical angle of an isosceles triangle also bisects the base.

8. Let A B be a given straight line, and from A let equal straight lines A C, A D be drawn making equal angles with A B on opposite sides of it; show that A B, produced if necessary, bisects C D at right angles.

9. The triangle A B C has equal sides A B and A C, and A D

bisects the angle B A C, the point D not lying in B C. Show that if lines D B E and D C F are drawn so that B E is equal to C F then the triangle A E F is isosceles.

10. The sides A B, A D of a quadrilateral are equal, and the diagonal A C bisects the angle D A B. Show that the sides B C, C D are equal, and that the diagonal A C bisects the angle B C D.

PROP. 5.

11. A B C is an isosceles triangle having each of the angles B and C double of the angle A. B D is drawn bisecting the angle B and meeting A C in D, shew that B D is equal to A D.

12. Two straight lines A B, and C D intersect in E and the lines E A, E B, E C, and E D are all equal. Show that the four angles E A D, E D A, E C B, and E B C are all equal.

Shew that the triangles B A C, D C A *are equal in all respects by Euc.* I. 4.

13. Apply the preceding proposition to show that when two straight lines intersect the vertical angles are equal, without assuming any proposition beyond the fifth.

14. In the quadrilateral A B C D, A B is equal to A D, and B C to C D; show that the angle A D C is equal to the angle A B C.

(To be continued.)

Our Whist Column.

By "Five of Clubs."

SKILL AT CHESS.

THE following from Cavendish's interesting "Card-table Talk" will be found well worth studying by Whist-players of all classes, good, bad, and indifferent:—

In the latter part of the winter of 1857, during an after-dinner conversation, it was remarked by some of the party that Whist is a mere matter of chance, since no amount of ingenuity can make a King win an Ace, and so on. This produced an argument as to the merits of the game; and, as two of the disputants obstinately maintained the original position, it was proposed to test their powers by matching them against two excellent players in the room. To this match, strange to say, the bad players agreed, and a date was fixed. Before the day arrived, it was proposed to play the match in double, another rubber of two good against two bad players being formed in an adjoining room, and the hands being played over again, the good players having the cards previously held by the bad ones, and *vice versâ*, the order of the play being, of course, in every other respect preserved. The difficulty now was to find two players sufficiently bad for this purpose; but two men were found, on condition of having odds laid them at starting, which was accordingly done.

On the appointed day, a table was formed in room A, and as soon as the first hand was played, the cards were re-sorted and conveyed into room B. There the hand was played over again, the good players in room B having the cards that the bad players had in room A. At the end of the hand, the result was noted for comparison, independently of the score, which was conducted in the usual way. Thirty-three hands were played in each room. In room A, the good players held very good cards, and won four rubbers out of six; in points, a balance of eighteen. In room B, the good players had, of course, the bad cards. They played seven rubbers with the same number of hands that in the other room had played six, and they won three out of the seven, losing seven points on the balance. The difference, therefore, was eleven points, or nearly one point a rubber in favour of skill.

A comparison of tricks only, showed some curious results. In seven of the hands the score by cards in each room was the same. In eighteen hands the balance of the score by cards was in favour of the superior players; in eight hands in favour of the inferior. In one of these hands the bad players won two by cards at one table, and three by cards at the other.

The most important result is, that at both tables the superior players gained a majority of tricks. In room A, they won on the balance nineteen by tricks; in room B, they won two by tricks.

It will be observed that this experiment does not altogether eliminate luck, as bad play sometimes succeeds. But by far the greater part of luck, viz., that due to the superiority of winning cards, is, by the plan described, quite got rid of.

Dr. Pole (the *Field*, June 16, 1866) arrives at a result nearly the same by a statistical method. He writes to this effect:—

"It is very desirable to ascertain the value of skill at Whist.

"The voluntary power we have over results at Whist is compounded of—1. The system of play. 2. The personal skill employed."

The modern system, which combines the hands of the two partners, as against no system (the personal skill of all being pretty equal), is worth—Dr. Pole thinks—about half-a-point a rubber, or rather more. About 900 rubbers played by systematic as against old-fashioned players, gave a balance of nearly 500 points in favour of system.

The personal skill will vary with each individual, and is difficult to estimate; but looking at published statistics, in which Dr. Pole had confidence, he puts the advantage of a very superior player (all using system) at about a quarter of a point a rubber; consequently the advantage due to combined personal skill (*i.e.*, two very skilful against two very unskilful players, all using system), would be more than half-a-point a rubber.

The conclusion arrived at by Dr. Pole is that "the total advantage of both elements of power over results at Whist may, under very favourable circumstances, be expected to amount to as much as one point per rubber."

Now, at play-clubs, nearly all the players adhere more or less closely to system, and the great majority have considerable personal skill. Consequently, only the very skilful player can expect to win anything, and he will only have the best player at the table for a partner on an average once in three times. It follows from this, that the expectation of a very skilful player at a play-club will only average, at the most, say a fifth or a sixth of a point a rubber.

J. R. W.—It is impossible to lay down rules for ruffing second hand; because so many varying circumstances may arise. The rule you mention, "only to ruff your adversary's lead" (meaning doubtless an adversary's suit), is quite insufficient, and also incorrect. In most cases the player to your right leads either his own suit or his partner's; but it by no means follows that you should in most cases ruff. The following is a good general rule, to which however there are numerous exceptions. If the trick is doubtful, that is, if you have no means of knowing whether your partner can take it or not, ruff if you have fewer trumps than four; if you have six trumps or more, ruff and lead trumps; if you have five trumps ruff and either lead from your strong suit or return your partner's: if you have four trumps pass the trick. The cases here to which there are most exceptions are the last two. It is often well to ruff a doubtful card even with the critical four trumps,—and with five trumps it is often desirable to pass the trick. Remember that when you are short in a suit led by an adversary, it is likely enough that your partner is long in it, and may be as much interested in seeing it established as the adversary who originally led it. So even if when you have passed, third in hand holds and plays the master card, your partner may reap the benefit. But of course in the greater number of cases, though a trick may in a sense be doubtful, the previous play has shown enough to make it more or less probable that your partner can take it; so that in reality the question of ruffing is generally a nice problem in probabilities.

NATIONAL HEALTH SOCIETY.—The eleventh annual report of the National Health Society, which was read at the Society's Rooms, 44, Berners-street, W. (Dr. Robert Farquharson, M.P., in the chair), shows that the society has carried on its work during the past year in a most practical manner. Hundreds of lectures on sanitary subjects have been delivered, not only all over the poorer parts of London, but in provincial towns, to large audiences of working men and women, classes of girls, district visitors, and others engaged in work amongst the poor. The society is much encouraged by the practical result of their lectures on "Keeping the House Healthy," "Rearing of Infants," "Prevention of the Spread of Infectious Diseases," "Preparation of Food," and kindred subjects, a knowledge of which is so much needed in our densely-crowded neighbourhoods. The question of poisonous dyes in domestic fabrics, of dust collection, of smoke abatement, and the prevention of cholera, have been investigated and reported upon by special committees appointed for the purpose. The Metropolitan Public Gardens and Playgrounds Association, which is in connection with the National Health Society, has been most active in assisting to open spaces for recreation in crowded parts of London, also in placing seats and planting trees in convenient situations. The Health Exhibition, held by the Society last June, was commented upon; and the Secretary stated that more than 100 members had joined the society during the year

Our Chess Column.

By Mephisto.

A BIT OF MORPHY.

We recollect an amusing incident which took place some years ago. Mr. Burns, of Liverpool—a very fine player—playing incognito, took the odds of a Knight from a well-known but weak professor, and lost purposely. "I think it is an advantage to play without the Knight," said Mr. Burns to his victorious opponent, "it allows yon to Castle early in the game. I think I will try and give you a Knight." So said, so done. Needless to say, Mr. Burns astonished his opponent by winning the game.

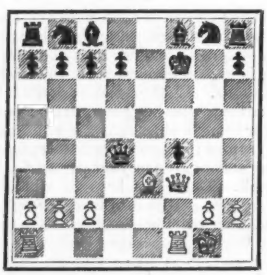
This week in playing and giving the odds of a Knight, we have actually had the strange experience of finding that in a certain variation it really is an advantage to be minus the Queen's Knight. This occurred in Morphy's variation of the Muzio Gambit, and came about as follows:

Remove White's Queen's Knight.

1. P to K4 / P to K4 2. P to KB4 / P takes P 3. Kt to KB3 / P to KKt4 4. B to B4 / P to Kt5
5. Castles / P takes Kt 6. Q takes P / Q to B3 (best) 7. P to K5 / Q takes P 8. B takes P (ch) /

This gives a very fierce attack, but it is hardly sound against a good defence. In giving the odds of a Knight it leaves White with three pieces to the bad, but in practice we have often succeeded in this bold venture:—8. / K takes B 9. P to Q4 / Q takes P (ch) 10. B to K3 /

Black.



White.

10. / Q to B3. Q to Kt2 seems better, but even then we think White could obtain a draw, as the following will show:—Q to Kt2. 11. Q to R5 (ch), Q to Kt3. 12. R takes P (ch), Kt to B3. 13. R takes Kt (ch)! K takes R. 14. B to Q4 (ch), K to B2. 15. R to B sq (ch) (this could not be done now but for the fact of White giving the odds of the QKt), K to Kt sq. 16. Q to Q5 (ch), Q to K3. 17. Q to Kt5 (ch), and White can draw by perpetual check if he likes. Also in the actual variation it can be seen that it is of great advantage for White to be able to bring his QR into play, whereby he limits Black's defences very much. After 10. Q to B3, the game continued as follows:—11. B takes P / P to Q3 12. Q to R5 (ch) / Q to Kt3 (The Black King could not play to K2 on account of B to Kt5, nor to K3 on account of QR to K sq (ch), threatening to follow up by Q to K8, mate! There is something to be said though in favour of K to Kt2. We think in that case, White would not do well to win the Black Queen by either B to R6 (ch) or B to K5, when, of course, the Queen could not take the B on account of R to B7, mate! but Kt takes B in the former case, and P takes B in the latter case would leave White no alternative but to take the Queen with his Rook, leaving his opponent with too many pieces for the Queen. Instead of either of these moves, White would do better to play B to K3, threatening to win the Queen for the Rook only, and with a better game, as she obviously cannot move to Kt3 on account of B to Q4 (ch). 13. B takes P (ch) / Kt to B3 14. R takes Kt (ch) / K takes R 15. Q to K5 (ch) / K to B7 16. R to B sq (ch) / which, of course, decides the game in favour of White, strange to say, owing to the fact of White having given the QKt, he is saved from being compelled to play Kt to QB3 in order to develop his Rook, in which case Black would gain time for a developing move which might repel the whole attack. If now K to Kt sq, the Rook mates; the game proceeded as follows:—16. B to B4 / 17. R takes B (ch) / Q takes R 18. Q takes Q (ch) / K to K sq 19. Q to K6 (ch) / K to Q sq 20. B to K5 / and Black resigned.

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LONDON (Brixton Hall) March 28, April 1, 4, 8 (1, 2, 3, 4).
" (Memorial Hall), March 24, 27, 31, April 3 (1, 2, 3, 4).
CRYSTAL PALACE, April 7, 9 (1, 3).
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Note.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*